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VOL. 16, NO. 1 March, 1941

THE QUARTERLY REVIEW
of BIOLOGY

THE AGASSIZ-ROGERS DEBATE ON EVOLUTION

BY W. M. SMALLWOOD

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THERE have been three notable debates on the general subject of what we now term "evolution." The first was between Georges Léopold Chrétien Frédéric Cuvier (1769–1832) and Geoffroy Saint-Hilaire (1772–1844). We shall refer but briefly to this controversy which stirred intellectual Europe.

The well-known Huxley-Wilberforce debate at Oxford was the third and most widely-publicized of the three. It was the least important as an analysis of the real problems involved, but probably carried more weight with the English-speaking world than either of the other two.

Chronologically, the Agassiz-Rogers debate began six months before the British Association met at Oxford. The following brief notes on the first and third debates afford a basis for comparison and the literature necessary for a more comprehensive analysis.

THE CUVIER-SAINT-HILAIRE DEBATE

It was on February twenty-second, 1830, that these two intellectual giants began their controversy in Paris before the members of the Académie de Sciences. Both were high-spirited, impulsive men, and leaders in the expanding field of comparative anatomy. Cuvier had formulated his catastrophic theory and his correlation theory. Saint-Hilaire was inclined to philosophize on the existence of a common basis of organization of all vertebrates, a philosophy which he extended to include the entire animal kingdom. This led him to make fantastic comparisons; and the following citation reveals how far his explanation departed from our modern ideas of genetic continuity: "In a word, we see here, whatever Bonnet and his followers may have said, nature pass from one plan to another, make a jump, leave between her productions a manifest gap. The mollusks are not intermediary to anything; they are the result of the development of other animals, and their own development has produced nothing superior to them" (1).

Cuvier and Saint-Hilaire had been friends and co-workers for a long time; but the speculations of the latter brought

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them to open argument when Geoffroy Saint-Hilaire read a paper on the "Organization of the Cephalopods," written by two young naturalists, Latreille and Meyranx. By this act, he gave his endorsement to their endeavor to show that the organization of cephalopod mollusks could be "assimilated," as they expressed it, to the vertebrates. We now consider the change they proposed as absurd: "They imagined a vertebrate to be bent double at the height of the navel, so that the ventral surface should remain outside and the two halves of the back, brought into contact, should be joined together" (2). This comparison between the squid (ink-fish) and a fish was more than Cuvier could endure in silence.

There is no complete record of exactly what was said in the several encounters between these two debaters. In Saint-Hilaire's mind, back of the comparisons of structures which were made, existed what he called *d'unite de plan*. This struck at the very basis of Cuvier's own classification of animals into four groups. "But what," Cuvier asked, "is *unity of plan*, and especially *unity of composition*, which is to serve henceforth as a new basis for zoology? These words evidently cannot be employed in the ordinary sense, in the sense of *identity*; for a polyp and even a whale, or an adder, do not possess all the organs of a man, placed in a similar way; the words unity of plan, unity of composition signify then simply, in the mouth of those who use them, *resemblance*, *analogy*" (3).

Saint-Hilaire then substituted *methodé des analogues*, and maintained that his explanation was entirely *new* and that Cuvier was holding that *structure* had been the basis of distinction since Aristotle's time. The method of analogies is independent of unity of plan, and has been used down to the present day. These discussions before the Académie lasted for a year, then were continued for two years longer in public lectures at the Collège de France (4).

This debate destroyed a friendship of long standing. Cuvier won before the Académie, but the man on the street was with Saint-Hilaire. The arguments presented by Saint-Hilaire comprised six problems that are not yet solved in their entirety: (1) the pre-existence in natural history of the genus; (2) the unity of organic composition; (3) the value of classification; (4) the fixity of species; (5) the final cause; and (6) the succession of organic life on earth (5).

These arguments, carried on over a period of three years, proceeded from the complex to the simple. Saint-Hilaire attempted to transform a fish into a squid. Man was the center of the philosophical universe; and the present-day historical or genetic method of thinking does not appear until we come to the Huxley-Wilberforce-Hooker and the Agassiz-Rogers debates.

THE HUXLEY-WILBERFORCE-HOOKER DEBATE

The summer meeting of the British Association was held at Oxford in 1860, and the first three days were devoted to the reading of papers. It had been rumored that the *Origin of Species* would come up for criticism. Hooker, the confidant of Darwin, did not attend the Thursday sessions, but wandered about the familiar streets. It was from his letter to Darwin that we learn of the beginning of the sharp differences of opinions, climaxing on Saturday in the debate in which he, Huxley, and the Bishop of Oxford participated.

During the informal discussion of Dr. Daubeny's paper, "On the final causes of sexuality in plants, with particular reference to Mr. Darwin's work on the Origin

of Species," the chairman called on Huxley who was reluctant to consider the general question of the truth of Darwin's theory. "He felt that a general audience, in which sentiment would unduly interfere with intellect, was not the public before which such a discussion should be carried on," (6) and that Dr. Daubeny had, in fact, brought forth nothing new.

Professor Owen then proceeded to give his personal opinions of similarities and differences between man and the monkey. Taking the brain of the gorilla, "it presented more differences, as compared with the brain of man than it did when compared with the brains of the very lowest and most problematical form of the Quadrumana. The deficiencies in cerebral structure between the gorilla and man were immense" (7).

The official report refers only briefly to Huxley's reply to Owen; but in one story of the meeting he is said to have retorted in this challenging manner: "I must directly and unequivocally contradict the statement. My procedure is unusual, but seems necessary. I shall justify it elsewhere" (8). This he did in his book, *Man's Place in Nature*, also in the *Natural History Review*.

The meetings on Friday were taken up with the reading of papers which held but passing interest. It was not until the Saturday program that a large audience was attracted to this sectional meeting of Zoology and Botany to listen to a contribution by Professor Draper of New York, "On the Intellectual Development of Europe, considered with Reference to the Views of Mr. Darwin and others, that the Progression of Organisms is determined by law" (9).

The room in which this section first assembled was so small that it was found necessary to adjourn to the Library of the New Museum; and that also became crowded to suffocation. There were several participants in the discussion of Dr. Draper's paper before the Bishop of Oxford began his address. It is very interesting to contrast the official report of the meeting with the personal accounts of some of the eye-witnesses. The mere fact that both Hooker and Huxley were urged to be present indicates that the opponents of Darwin had made extensive plans to "smash Darwin" (10), for which purpose they had selected "Soapy Sam" (11) Wilberforce, as the Bishop of Oxford had been known when an under-graduate. Samuel Wilberforce spoke for half an hour, and had evidently been coached by Owen. He received great applause from his adherents who occupied the center of the auditorium. At the close of his address, he made reference to Huxley's genealogy in words which apparently no one took the trouble to record accurately, although the question implied that one of Huxley's grandparents must have been related to an ape. There are two or more versions of the phrasing of Bishop Wilberforce's question, and we find the same confusion in regard to Huxley's reply:

> I asserted, and I repeat, that a man has no reason to be ashamed of having an ape for his grandfather. If there were an ancestor whom I should feel shame in recalling, it would be a *man*, a man of restless and versatile intellect, who, not content with an equivocal success in his own sphere of activity, plunges into scientific questions with which he has no real acquaintance, only to obscure them by an aimless rhetoric, and distract the attention of his hearers from the real point at issue by eloquent digressions, and skilled appeals to religious prejudice (12).

Huxley was followed by Hooker, the botanical friend of Darwin, who had been confidentially told about natural selection as far back as 1844—a fact which gave him a decided advantage in the argument. Hooker had been very reluctant to enter the controversy until the Bishop's address had revealed his unfairness, as we

learn from this unpublished latter to Asa Gray:

> I spoke only once, the last of all; showed that he [the Bishop] could never have read Darwin's book and exposed ignorance of the rudiments of science—I shut him up completely, *he had not a word to reply*, and the discussion was closed amid rounds of applause for my side (13).

When we compare this debate with that between Cuvier and Saint-Hilaire, we realize that it was not so much a discussion between two eminent scientists as a struggle between science on the one hand, and the church on the other. Huxley gained great notoriety for his retort, while the Bishop's statements have brought him lasting ridicule. Though the understanding and importance of natural selection was not appreciably advanced by the manner in which this highly-emotional controversy was conducted, it is the most widely-quoted of all debates over Darwin's theory. The good sportmanship of the English gave a hearing to Huxley and Hooker, and the resulting publicity served to popularize evolution.

THE AGASSIZ-ROGERS DEBATE

Some may object to designating the successive discussions at the regular meetings of the Boston Society of Natural History (February 15, March 7, March 21, and April 4, 1860) as a debate. Though not announced as such in advance, the zeal with which it was carried on for months seems to justify this interpretation. The antagonists were that widely-known scientist, Louis Agassiz, and William Barton Rogers, later to become President of the Massachusetts Institute of Technology. Agassiz's popularity as a lecturer was conceded, and his winsome personality aided in the acceptance of his leadership. Rogers had left an enviable reputation in Charlottesville, Virginia, where he was highly regarded by the students during his eighteen years as professor of natural philosophy.

The personal charm and scientific attainments of Louis Agassiz are well known. If we are to enjoy this famous contest, it is desirable to outline some of Rogers's qualities and achievements. Professor Rogers had been doing pioneer research in geology for thirty-two years and, with his brother Henry, had initiated geological surveys in Virginia, Pennsylvania, and New Jersey. The history of American geology places their names in the front rank:

> By 1844, the Rogers brothers had planned their nomenclature and classification of the Appalachian Paleozoic and later formations. They recognized that, in some places, Paleozoic rocks exceed 30,000 feet in thickness, are made up of a variety of sediments, and represent a profound lapse of time. Their subdivisions of the Paleozoic and later formations were made on the fossil content, mineral character, and relative expansion of the formations. This scheme was followed for many years, and, as late as 1900, it had not been completely abandoned (14).

When the English edition of the *Origin of Species* was first published, Henry Rogers was in Scotland. The following brief extracts from the letters exchanged between William and his brother, Henry, give an intimate picture of the attitude of these two Scottish-Americans toward natural selection, even before the American edition of the *Origin* was brought out by Appleton, in January, 1860.

Henry Rogers, writing to his brother from Glasgow, on December 23, 1859, expressed his opinion of the *Origin* in these words:

> The only matter of any interest is the appearance of Charles Darwin's book, *On the Origin of Species by Means of Natural Selection*. It is a suggestive book, full of ingenious arguments in favour of the Lamarckian hypothesis. Huxley, who bitterly criticised the 'Vestiges,' has reviewed this work in terms of high commendation. When you read it you will

often say, I think, that in his geology Darwin outdoes Lyell himself in ignoring paroxysmal actions. This is its chief blemish with me.

On January 2, 1860, William replied from Boston:

I have been reading the early chapters of Darwin's book with great interest. . . . The more I look into Darwin's argument the more I like it, save in the one particular of ignoring entirely violent and sudden physical changes. The calmness and truth-loving spirit of the book are truly admirable. Much of it I know *you* will approve.

Later in that same month William again wrote:

The next number of 'Silliman' will contain an elaborate review of Darwin's book by Gray, who called some days ago to leave his proof sheets with me. He has not affixed his name, and requested me to say nothing of it for the present. I hear that Mr. Ticknor has just heard from Lyell, who speaks approvingly of Darwin's views. I anticipate many disciples for Darwin on this side of the Atlantic, in spite of the '*disastrous*' tendency of his views.

Lyell, I hear, is preparing another edition of his 'Elements,' and I presume he will introduce this subject, and the late evidences of the antiquity of man, as well as other new matter into it. . . .

How I long, my dear brother, to talk with you about these scientific matters. I have never felt so much the want of such communion.

When Henry replied to his brother's letter on February 24, 1860, he said:

I wrote to Huxley awhile since in relation to Darwin's book (15) and your liberal defence of it, and he replies much pleased. In a few years, opinion among the reflecting in Europe will be with Darwin, you may depend on it, as to the law of a natural selection. As to the other point, development of species from species, firmly as I believe in it, I think it will never be capable of a strictly scientific proof, no more can the opposite doctrine of supernatural creations, and therefore the main point to insist on now is toleration and no dogmatizing (16).

Thomas Henry Huxley, writing to Henry Rogers from London, on January 9, 1860, asked "Have you read Darwin?" He prophesied: "There will be great fighting about his views for the next ten years, and great things will arise out of the combat." In another letter, written on the seventeenth of the same month, he told Rogers: " 'Darwin' is the great subject just at present and everybody is talking about it" (17).

The historians of this period agree in describing both Agassiz and Rogers as unusually brilliant men as well as in their statements that the man who finally triumphed did not permit his emotions to betray him.

Let us now turn to this least-known but ablest of the three great debates over evolution.

The First Meeting, February 15, 1860

When the Boston Society of Natural History was called to order on February 15, 1860, Dr. C. T. Jackson, the Vice-President, was in the chair. The American edition of the *Origin of Species* had been off the press scarcely three weeks; but it is certain, from the letters of Rogers, that copies of the English edition had been available for some time.

Louis Agassiz opened the meeting with an attack on Darwin's "ingenious but fanciful theory." He supported his objections against variation by calling attention to the exceedingly long existence of the brachiopod, *Lingula*, which had not undergone any variations in practically its entire geological life span. Thus, with scientific data that seemed conclusive, the debate began. The record of this exchange of arguments is told in the language of the secretary, and not exactly as phrased by the contestants. The minutes of that first meeting report Agassiz as beginning in this fashion:

Mr. Darwin he acknowledged to be one of the best naturalists of England, a laborious and successful writer; his works on the coral reefs, on the cirripeds, and his narrative of the voyage of the Beagle, show

him to be a skilful and well prepared naturalist; but this great knowledge and experience had, in the present instance, been brought to the support, in his opinion, of an ingenious but fanciful theory. According to Darwin, the primary cell, by a process of differentiation and gradual improvement by natural selection, has produced all the diversities of animals, in geological and present times. He did not think it fair to compare the present fauna of the world with the fauna of any geological horizon as known in one locality; and he thought this method of comparison had led to this idea of gradual development (18).

Rogers replied to Professor Agassiz's objection over animals that persist for a long period without change, and

admitted that the persistency of *lingula*, and other similar cases that might be adduced, were formidable objections to this theory; but he thought that Darwin would meet such objections by the fact that the vital characters of some animals fit them for resisting change and extinction better than more plastic natures; from our knowledge of domesticated animals we find that dogs have changed very much, and that cats have changed hardly at all; some have great energy of resistance, and some very little (19).

The argument presented by Agassiz continued to be a stumbling-block for a long time; for we note that when Huxley, who had been expounding Darwinian evolution for seventeen years, visited the United States, he devoted considerable time to "persistent types" in his lecture on evolution. "It is obvious," said Huxley, "that, if it can be proved that animals have endured, without undergoing any demonstrable change of structure, for so long a period as four thousand years, no form of the hypothesis of evolution which assumes that animals undergo a constant and necessary progressive change can be tenable; unless, indeed, it be further assumed that four thousand years is too short a time for the production of a change sufficiently great to be detected" (20).

Huxley then listed the several fossil species known to have been in existence for hundreds of thousands of years, and continued:

Facts of this kind are undoubtedly fatal to any form of the doctrine of evolution which postulates the supposition that there is an intrinsic necessity, on the part of animal forms which have once come into existence, to undergo continual modification; and they are as distinctly opposed to any view which involves the belief, that such modification as may occur, must take place, at the same rate, in all the different types of animal or vegetable life. The facts, as I have placed them before you, obviously directly contradict any form of the hypothesis of evolution which stands in need of these two postulates (21).

This was really a problem in variation; one that was not fully analyzed until the discussions on Mendelism and mutation. But, to return to the debate; Rogers next raised the question of migrations and possible re-migrations of extinct animals, a problem brought to the foreground when biogenesis became the accepted law of life and older explanations were no longer satisfactory. Rogers took a bold step, striking at one of Agassiz's most cherished views, when he applied this idea to his theories of animals that lived in the ancient world.

On the coast of Virginia and Maryland there is an extensive oyster-bed, but which has not been continuous through all time; at one time the oysters disappear, and clams make their appearance; the latter disappear, and oysters reappear; these he regarded as instances of emigration and remigrations over great spaces.

To this statement Professor Agassiz replied that: "As to these alleged migrations, we know that species are well circumscribed within the limits of faunæ; and . . . before such a line of argument can be followed, it must be shown that any species pass from one continent to another, except from man's agency" (22).

Rogers "inquired of Prof. Agassiz if any vertebrate had ever been found in strata lower than the upper Silurian"

(23); and Agassiz's reply summarized the widely-held belief regarding ancient forms of plants and animals. There was no basis for compromise in his assertion that "in this lowest system of fossils there was such a coordination of the animal series as shows that all its great and principal classes were then existing" (24).

The scientific argument was temporarily interrupted as the qualifications of Darwin were questioned by one of Agassiz's old friends, Emerson, who had failed entirely to understand Darwin's method:

> Darwin comes before the reader at once as an advocate of a seemingly foregone conclusion, and argues, not for the purpose of finding in what direction the evidence of any particular fact would lead the mind, but for the purpose of finding something in the fact favorable to his preconceived opinion. Admitting the difficulties in his theory, he tries to explain them away by various suppositions and *ifs*, which by frequent repetition and consideration seem in the mind of the author to become established truths, and are used as arguments (25).

In correcting Mr. Emerson, Professor Rogers stated that:

> . . . the present work of Darwin is a *résumé* of his conviction on the subject, without the presentation of the facts upon which it rests, which he has not had time to arrange. The problem is admitted to be of transcendent difficulty, and such as no observer or theorist can hope now or perhaps ever positively to resolve. Mr. Darwin makes no pretensions to an absolute demonstration, but, after an impartial survey of the facts bearing on the subject and a candid appreciation of the opposing considerations, adopts the view set forth in his book, as offering, in his opinion, a more rational and satisfactory explanation of the history of living nature than the hypothesis of innumerable successive creations. Prof. Rogers regarded the work as marked in an extraordinary degree by fairness in the statement of opposing as well as favorable arguments, by the absence of dogmatism, and by all other evidence of a truthloving spirit, as well as by the extent and variety of its knowledge and the breadth of its philosophical views (26).

Agassiz contended that the embryo was superior to the adult and that, in turn, the egg was more perfect than the embryo. This strange line of reasoning grew out of the necessity for explaining the presence of new organs in one of the four great groups of animals. Since the entire vertebrate group, for example, was related, such new organs as teeth, wings, lungs, etc., presented a difficult problem to the Creationist. Agassiz met this problem by attributing to the egg a complexity greater than that of the adult. Rogers took advantage of this unscientific proposition, thus placing Agassiz on the defensive. In the following manner Rogers carried his argument to fossil life:

> As regards the statement that the most ancient types of life were higher or more perfect than recent ones, he had always considered Prof. Agassiz as maintaining that these earlier forms were of an embryonic character; and in this connection he remarked that the term 'perfection' is just as indefinite as the word 'species.' He considered perfection as specialization in each type; if an animal approach nearer perfection because, for instance, it be part fish and part reptile, or if a structure part animal and part vegetable be more perfect than the plant, then is the cell the type of perfection, combining as it does properties belonging to both kingdoms; he considered perfection, not the union of different types, but specialization in each particular type.

The recording secretary, Samuel Kneeland, indicated that: "Prof. Agassiz considered perfection to mean an embodiment of the highest combinations, the most complex representation of life. The embryo fish presents features of its type superior to those of the adult fish; the tendency to specialization increases with its growth, and the animal at last becomes only a fish, losing its embryonic type of the higher vertebrates. As a generalization or philosophic conception, the vertebrate egg is superior to man himself, inasmuch as it embodies all that may be produced from it (27).

Thus the first meeting was concluded. Never before had Agassiz met an

opponent who was his equal as a scientist and his superior as a logical thinker. He was annoyed when members of the audience interrupted him with questions or shook their heads in disapproval. These minor irritations were something that the great Louis Agassiz was unaccustomed to, and may account for some of the loose statements which led to his downfall—a defeat clearly foreseen at the end of the first day's arguments (28).

Agassiz was a poor debater, and often lost patience as this incident reveals. There was a Scientific Club consisting of several members of the Harvard faculty which held frequent meetings. At one of these meetings Asa Gray argued so pointedly against Agassiz that he became sufficiently angry to challenge Gray to a duel (29). These two men also encountered each other in arguments at the meetings of the American Academy of Arts and Sciences, and we note that Gray's retorts were given extensive recognition in England (30).

The Second Meeting, March 7, 1860

The Vice-President was in the chair. Mr. C. H. Hitchcock explained his map of the principal features of the complicated geology of the State of Vermont. Professor Rogers discussed the new facts revealed in Hitchcock's map, and asked permission to incorporate into the minutes a paper "On the Geological Structure of Western Vermont," which he had read at the American Association at Albany in 1851. The reading of these papers was followed by the presentation of a section of a tusk of an elephant and an explanation as to how it was formed; after which Rogers demonstrated the laws of fracture. The above incidents are recorded as indications that Rogers might have been ready to allow the question of Darwinism to rest. This, however, was not what Agassiz desired. He had come to this meeting with a carefully-prepared paper on consecutive faunæ and their geological formations. He objected to the argument from "domestication" as an explanation of similar changes in geological ages, and argued that representatives do not pass from one fauna to another. He presented his own definition of faunæ as:

> . . . groups of animals enclosed within circumscribed areas; there are many of these on the globe, and they must not be confounded with zoölogical realms; of the latter, New Holland may be mentioned as an example, having animals of a peculiar type; so are the East Indies, Africa south of the Atlas Mountains, America from the sub-arctic regions to Patagonia, and the arctic regions themselves. Faunæ, on the other hand, occupy more limited provinces characterized by species related to each other, as they are more largely in realms. Faunæ differ in various parts of the world, and no one can be taken as a type of existing creation; for instance, the fauna of Canada differs entirely from that of Africa, and any zoölogist who should take one or the other or any single fauna as the type of the world's animals would commit an absurdity; yet geologists do this constantly in their identifications of geological periods, and of course fall into the gravest errors. He found fault with the methods of determining the limits of successive faunæ usually pursued by geologists; he thought that this order should be determined by the fossils; that the rocks should be regarded merely as the tombs of the fossils, that naturalists should try to find out the animals of an epoch, and establish the limits of faunæ on zoölogical and not on physical principles (31).

This gave Professor Rogers his opportunity; and he took full advantage of it. The question was: "Did species pass from one strata to another?" Before this line of reasoning presented by Agassiz could be accepted, the entire severance of these faunæ must be demonstrated. What were the facts?

> In New York, out of more than seventy forms found in the strata below the upper limit of the Black River limestone, only three have been observed by Prof. Hall to pass up into the overlying Trenton

limestone, and on the same horizon a transition almost as abrupt shows itself in parts of Pennsylvania and Virginia. Here then we would seem to be justified in drawing a strong line of separation between the contiguous Black River and Trenton faunæ. But turning to Canada, we find a very different distribution of the fossils. In this region the Black River limestone, that is, the rock containing the characteristic Black River fossils, includes a preponderating number of species found also in the Trenton. According to the Canada paleontologists, fifty-two out of seventy-five are common to both formations, and what is still more interesting, some Trenton species are found in the yet lower group of the Chazy. Here obviously we can no longer draw a limit between the Black River and Trenton faunæ, but must blend them gradationally into one (32).

Seeing, then, that faunæ are not unfrequently mingled in contiguous formations, that they do not hold the same precise geological level or vertical distribution at different localities, that they may even become inverted in relation to each other, offering in this and other cases evidence of emigration and re-migration in successive times, Prof. Rogers maintained that the *precise and absolute limitation* of faunæ to formations, as advocated by Prof. Agassiz, if true in any case, is at best but of local and partial application, and can not be the basis of a paleontological arrangement of formations. . . . In regard to the discrimination of species, the question at last must come to this: What is the limit of specific difference? who shall be the arbiter? what the principle of distinction between species and variety, and what the guide in drawing the lines of demarcation of the successive faunæ?

Prof. Agassiz remarked that he did not expect the immediate reception of his views, though convinced that they were true, but believed that after mature examination of his facts they would be generally received (33).

Agassiz then took an illustration from chemistry, and Rogers again turned the argument against him:

He alluded to chemistry, in which there was a time when platinum and other silver-like metals were not distinguished from silver; but in the progress of science they were ascertained to be separate and distinct species.

Prof. Rogers replied that this argument would answer equally well for the other side of the question; for instance, chemistry has reduced to one many supposed different species, as the diamond, plumbago, and carbon (34).

The meeting then turned to examine an exhibition of forty Brazilian birds; and, later, to elect new members to the Society.

The Third Meeting, March 21, 1860

The third meeting, on March 21, 1860, opened with President Jeffries Wyman in the chair. The first communication was a description of the color of the inside of fresh-water clam shells. In the two previous meetings, the arguments raised the important question of the stratigraphical relations of deposits formed in an ocean. Rogers gave his views under "stationary," "subsiding," and "rising." The critical manner in which Rogers met the argument of Agassiz places this debate in the front rank of scientific evolutionary discussions.

Both Agassiz and Rogers should have been familiar with Lyell's ideas on this subject, as he had phrased them, in 1842, in this way:

But if the bottom be lowered by sinking at the same rate that it is raised by fluviatile mud, the bay can never be turned into dry land. In that case one new layer of matter may be superimposed upon another for a thickness of many thousand feet, and the fossils of the inferior beds may differ greatly from those entombed in the uppermost, yet every intermediate gradation may be indicated in the passage from an older to a newer assemblage of species (35).

Professor Rogers revealed that he was entirely familiar with this problem. In fact, Merrill (36) paid tribute to the excellence of the Rogers brothers in their work on fundamental problems relating to the physics and structure of mountain building. Evidence that the question of subsidence was being discussed prior to the Agassiz-Rogers debate, can be seen in the abstract of a paper on the subject read before the Montreal meeting of the A.A.A.S. by Professor G. H. Cook (37), of New

Jersey. Even as early as 1840, J. D. Dana offered a solution to the question of continental uplift and depression.

The following is a sample of Rogers's argument:

> In the first and second of the conditions here named, the level of the resulting land would be *approximately horizontal;* while in the third case, that of the uplifting of the ocean-floor during the accumulation of the deposits, the surface would present a slope descending from the oldest deposits on the first shore-line to the strata latest formed; in other words, the *older deposits* would crop out at the *higher level*, and the successively later ones at a less and less elevation.
>
> The Appalachian strata embraced between Lake Ontario and the Pennsylvania coal region present a relation of levels *the reverse of that last named*, the older strata cropping out at successively lower levels as we proceed northward, while the newer formations, the Devonian and Carboniferous rocks on the south, are piled up to a height of some thousands of feet above the level of these outcrops. For this and other reasons, Prof. Rogers could not admit the theory which regarded the present stratigraphical features of this region as evidence of a deposition of the strata during a long-continued upward movement of the ancient sea-floor (38).

It is interesting that the debate should finally turn to the Catastrophic Theory of Cuvier, a view that had been widely accepted. Within a few weeks after this meeting, Rogers presented a direct challenge to this theory of fifty years' standing in his study of the denudation of rocks:

> This existing configuration has undoubtedly been the work of subsequent denudation, of which extensive and unmistakable evidences are apparent throughout the paleozoic area. The theory of an uplifting movement during the deposition does not, as has been supposed, dispense with the necessity of such a further agency for remodelling the surface. On the contrary, in this case, the depth of denudation required to carve out the profile of the region in question, so as to make it conform to the existing features, would be far greater than would be needed to bring the imaginary subsidence-area to a like agreement. In the latter conditions, the denuding force would be called on to remove only a certain amount of material below the horizontal surface over the northern portion of the tract; in the former, it would have first to cut down the greatly elevated outcrops at the north to bring them on a level with the southern deposits, and after this to do an additional amount of excavation equal to that of the other surface (39).

Agassiz, on his part, displayed a lack of first-hand knowledge. The only reply he made was to re-state his conviction that "there was not subsidence during the deposition of the New York strata, and that the facts do not indicate it, but rather an upheaval" (40).

The meeting then listened to a letter from Liberia, Africa; but when this was finished, Agassiz immediately returned to the argument and attempted to answer Rogers's statements on migration.

> His object at the present time was not to explain the origin and connection of faunæ; we must take them as we find them, as matters of fact, without reference now as to how they were produced. It is important in the discussion of this subject to bear in mind that some faunæ are strictly defined, while others run together; there is very great difference in this respect; Mr. Wallace, a strong advocate of Darwin's theory, admits the remarkable limitation between the Australian and Indian Archipelago faunæ, separated by a strait only fifteen miles wide, yet, with the exception of a few birds, entirely distinct. Faunæ are not necessarily, therefore, like each other because near together, nor unlike because widely separated; the former is shown by the case just cited, in which there is complete distinction, though circumstances favor a mingling of faunæ; on the other hand, those of widely distant Africa and the east coast of America between the tropics are very much alike. These do not look like migrations, which are at best limited, and in which, if the conditions of life were much changed, the animals would be destroyed; marine animals, in an element which invites migration, are very much circumscribed within limits as to depth of water, and could not migrate from one part of the world to another across ocean abysses. Another obstacle in the way of migration is the transfer of progeny; eggs in most animals cannot bear much change of temperature or of location, without destruction of the contained embryo. . . .
>
> In regard to the vitality of eggs, the President [Wyman] remarked that it is well known that the eggs of salmon and trout are sent by *diligence* all over

France; and Rogers observed that the flight of most birds would enable them to multiply on both sides of a narrow strait like that alluded to (41).

The Fourth Meeting, April 4, 1860

The fourth meeting was held on April 4, with President Wyman in the chair. Agassiz opened the proceedings with the statement that he did not see evidence of subsidence and denudation, as suggested by Rogers.

He exhibited a series of fossils to show the distinctness of faunæ, explaining why it is not always correct to identify geological periods by the identity of fossils; two questions are involved in the examination of this subject, one that of time or of period, the other that of space or limitation of faunæ.

Preliminary to the principal topic, he stated that he knew no such thing as a variety in the animal kingdom, except such as are stages of growth, within the limits of species; he instanced as an example one of the meandrine corals (*Manicina*), and other polymorphous types, which come within this law; in 1200 echini which he had examined and carefully studied, he had not found a single variety which did not arise from an imperfect stage of growth; so in 6000 fishes, he had not seen a variety except in coloration, which he had before shown was connected with their growth. So that he would start with the propositions that animals do not vary, and that species remain within the limits of their type (42).

Rogers repeated and extended his arguments of the previous meeting:

Thus, therefore, on no hypothesis of a *secular rising* of the sea-bottom can we explain the formation of our Appalachian paleozoic deposits. On the other hand, considering their aggregate thickness, as well as their continuity, composition, and stratigraphical arrangement, we are entitled to conclude that they were accumulated during *a long period of subsidence of the ocean-floor, varied by many and long pauses and upward oscillations* (43).

Agassiz's reply must have been made under emotional stress, for he conceded upheaval of the shore, though he refused to acknowledge denudation:

Prof. Rogers replied that he had listened with extreme surprise to the statement just made by Prof. Agassiz, disclaiming the theory of the rising of the ocean-floor during the formation of our northern paleozoic strata. According to his recollection, the discussion was commenced by Prof. Agassiz's denying the correctness of the views of Darwin and others of the extensive destruction of strata and their fossils during a period of slow upheaval, and urging as an insuperable objection the great extent and completeness of the paleozoic series of New York, which he maintained had been deposited during a *period of upheaval*. As, however, Prof. Agassiz has now stated that he recognizes the *subsidence* of the ocean-bed as essential to the theory of their formation, Prof. Rogers thought it of no importance in this connection how that depression may have been brought about, or whether it was accompanied by a stationary or a rising condition of the ancient shore (44).

Such is the record of the conclusion of this debate between Louis Agassiz and William Barton Rogers in the *Minutes* of the Boston Society of Natural History.

One naturally wonders how this debate influenced public opinion. A search of the Boston newspapers issued during January, February, March, and April of 1860, has thus far failed to show that it was even mentioned.

The author will welcome any criticisms or additions before this paper becomes incorporated into a book on the history of biology in the United States.

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FORM AND FUNCTION IN THE SLOTH

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INTRODUCTION

"SPEED, speed and still more speed" began a newspaper editorial recently, considering the impulses of civilized man. Regarding simple rate of physical movement, however, the distance-in-time curve for man and most of his interests has now become rather flattened at the higher levels. It may be interesting to observe in contrast a lowlier form, but one still in our own mammalian class, the proverbially tardy sloth. Its curve of activity, after many aeons of concentrated specialization, has become almost asymptotic at a very low level. But withal its biological restrictions the sloth has achieved in some ways much freedom.

The opening of an inter-American highway through Panama will allow the 60-miles-an-hour motorist to meet the 6-hours-a-mile sloth. It will be one of the most unique creatures to be found in the tropics. Loafing lazily in nearby trees, or crawling occasionally with many pains and much difficulty across the road, this blissfully inhibited tardigrade may prompt a refreshing and rewarding pause. It will furnish an amazing object lesson in leisurely living.

Almost wholly the three-toed species will thus be observed by the roadway, for the two-toed animal gives the deadly *Homo* a wide berth. The deep curiosity and normal absence of fear which are shown by the three-toed *Bradypus* lead it into strange highways and byways in life. Somewhat like the inquisitive and voracious opossum, it may now and then be found almost within the precincts of one's culinary quarters. Yet relatively few people to date, even in the tropics, have met the humble sloth. On a well-travelled tropical highway, however, there will soon occur a great loss by slaughter of these animals—totalling many more than were ever consumed by the hungry native or utilized by the zealous scientist. Happily their great number and relative inaccessibility in the forest will prevent extinction of a highly interesting and edifying mammalian form.

HISTORICAL

Early accounts of the sloth are given by a number of writers, although they interest one more often by their quaint description than by factual exactness. One of the most interesting and apparently the earliest reference is that of Oviedo y Valdés (1526). Following several visits to many parts of the Americas, and gold-smelting operations in San Domingo, this Spanish knight and historiographer contributed classical and fairly trustworthy accounts of his travels. His book issued in summary form in Toledo over 400 years ago represents his first published natural history of the Indies, and from it the following delightful account of the sloth is derived.

[The author is greatly indebted to Professor Julio S. Galban of the University of Virginia for the translation here given from Oviedo's rare "Historia," in the University's Tracy McGregor Library. It may be observed that an effort has been made to retain the spirit as well as the letter of the author, in making the translation.]

Little Quick Pedro is the stupidest animal that can be seen in the world. So slow and heavy is he that it takes him all day to go fifty paces. The first Christians who saw this animal (remembering that in Spain the negro is usually called John White, so that it may be understood conversely) as soon as they set eyes on him gave him the name opposite to his nature: because he was extremely slow, they called him quick. This is one of the strange animals, very much seen in the mainland, and very different from all other animals. He must be as long as two spans when fully grown; very little more than this measure if somewhat over-grown: any smaller ones that are found are young. They are almost as broad as long; and they have four thin feet, and on each hand and foot four long nails, like those of birds, and joined: but neither the nails nor the hands are such as he can support himself on them, and because of this, and because of the slightness of his arms and legs and the weight of the body, he carries his belly almost dragging on the ground. His neck is high and straight just like a pestle of equal width to the end, without making any difference in width between the head and neck; and at the end of that neck he has a face almost round, very similar to that of an owl, and the hair itself makes a ring around his face, a little longer than wide; and his eyes are small and round, and his nose like a monkey's, and his mouth very small, and he moves his neck from one side to the other as if astonished; and his intention, or what he seems to try and desire the most is to hold on to a tree, or anything that he can climb, and so most of the times that these animals are found they are taken in trees, on which climbing very slowly they go clinging with their long nails. Their hair is between light and dark, almost the very color of the hair of a weasel, and they have no tail. His voice is very different from that of all the rest of the animals of the world: because it sounds only at night, and as a whole, in continued chant, from time to time, singing six notes one higher than the other, always descending: so the highest note is the first, and from that one he descends, lowering his voice, as one might say, la sol fa mi re do, thus this animal says ha ha ha ha ha ha. Without doubt it seems to me that, as I said in the chapter of the Armadillos, these animals might have been the origin or hint to make the armour of horses; so, hearing this animal, the first inventor of music could have had a good base to give it beginning, better than any in the world, because the said Little Quick Pedro teaches us by his six notes the same as can be understood by la sol fa mi re do: returning to my story, I say that after this animal has sung at short intervals, he sings the same thing again. This he does at night and never in the daytime, and because of this and because he is near-sighted it seems to me that he must be a nocturnal animal and a friend of darkness. Whenever Christians take this animal, and bring him home, he goes around at his own gait; and neither by threat, blow, nor prodding does he move any faster than he is accustomed to do without tiring: and if he encounters a tree he goes to it and climbs to the highest of the branches, and remains in the tree, eight and ten and twenty days, and it cannot be known or understood what he eats. I have had him at home and what I was able to understand of this animal is that he must sustain himself on air; and of my opinion I found many in the land, because he has never been seen to eat a thing but continuously to turn his head, or mouth, towards the wind, more frequently than to any other part, by which it is known that the air is very agreeable to them. He bites not nor can he do so, since his mouth is extremely small, nor is he poisonous; nor have I ever until now seen an uglier animal, or appearing as useless as this one.

The illustration of the three-toed sloth from Oviedo's history (Fig. 1) represents the animal as if placed on the ground in a sitting posture like that assumed by a young child. This unique attitude is one which the animal may to be sure maintain momentarily: in any case it is true that the sloth presents an extremely quaint picture whether viewed sedant or scandent.

One of the early buccaneers, Exquemelin (1678), recounts that a Captain Sharp and his crew caught a sloth on a small island (the Isle of Gorgona or "Captain Sharp's Isle"), and commented that it was a beast "well-deserving that name."

Captain William Dampier's travels around the Bay of Campeachy and other parts brought him into contact with the humble sloth. Some of the animal's habits are described interestingly, with

not unusual over-emphasis. In his "Voyages" (1697) he remarks that

"They are very mischievous to the Trees where they come. . . . They never descend till they have stript every Limb and Bough, and made them as bare as Winter. It takes them up eight or nine Minutes to move one of their Feet three Inches forward; and they move all their four Feet one after another, at the same slow rate; neither will stripes make them mend their pace; which I have tried to do, by whipping them; but they seem insensible, and can neither be frighted, or provoked to move faster" (Masefield edition, 1906).

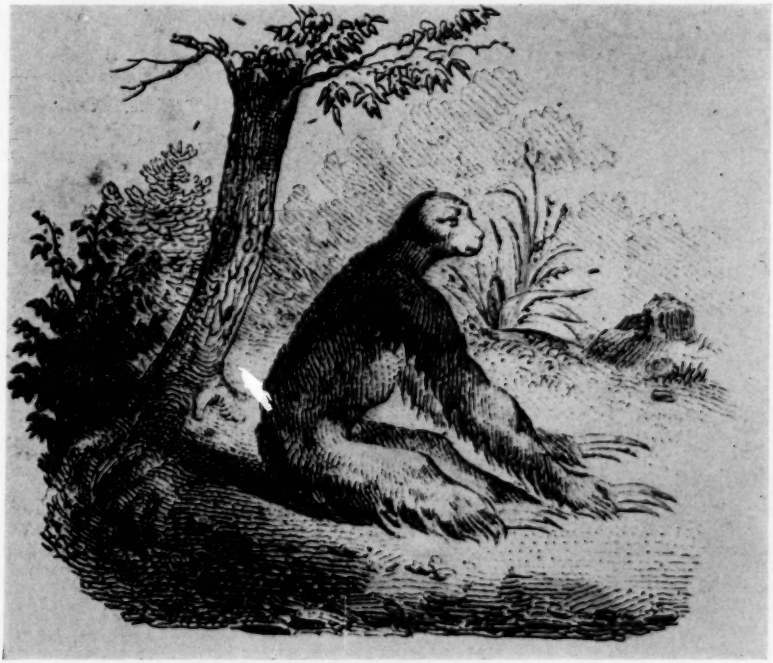

FIG. 1. PROBABLY THE OLDEST PICTURIZATION OF THE THREE-TOED SLOTH (OVIEDO Y VALDÉS, 1526)
The unsupported sedant position is really an impossible one for the sloth, and the not unusual anthropomorphic representation is evident.

The comments of Don Juan and de Ulloa on the sloth are more fanciful. These Spanish sea-captains, it may be recalled, visited the New World in 1735. A short excerpt is taken from the translation of their work (1748) by Adams (1807):

It resembles a middling monkey, but of a wretched appearance, its skin being of a greyish brown, all over corrugated, and the legs and feet without hair. He is so lumpish, as not to stand in need of either chain or hutch, for he never stirs till compelled by hunger. . . . The food of this creature is generally wild fruits; when he can find none on the ground, he looks out for a tree well loaded, which, with a great deal of pain, he climbs: and, to save himself such another toilsome ascent, plucks off all the fruit, throwing them on the ground; and to avoid the pain of descending, forms himself into a ball, and drops from the branches. At the foot of this tree he continues till all the fruits are consumed, never stirring till hunger forces him to seek again for food.

Buffon (1766) has described the broader features of the anatomy of sloths, including the stomach and intestines, sex apparatus, teeth and bones, as well as many external characteristics. He also remarks

that the Marquis de Montmirail kept a two-toed sloth for three years in a menagerie, and that it fed on bread, apples, milk,

Interesting observations have also been made by Oliver Goldsmith (1825). An animal that is slow from necessity, he

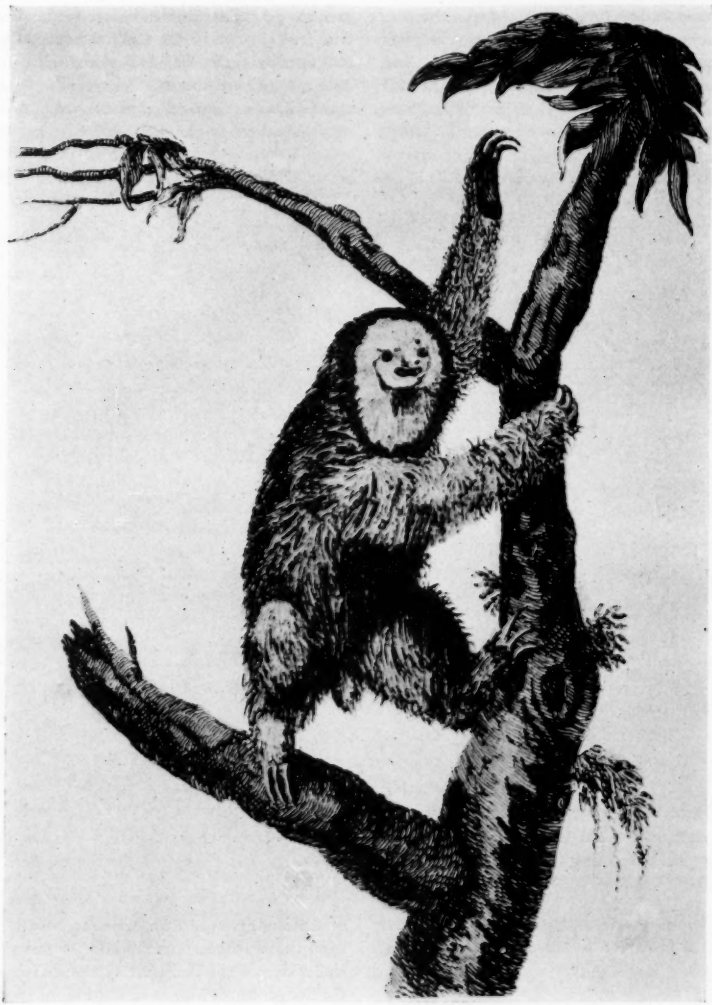

FIG. 2. A RATHER LIFE-LIKE ILLUSTRATION OF THE TRIDACTYL SLOTH FROM PENNANT (1771)

and "poisonous leaves." The illustrations by Pennant (1771) and by Buffon (1776) are rather imaginative (Figs. 2 and 3).

remarks, the sloth may move three feet an hour when impelled by the severest stings of hunger. He considered it represented an unfinished production of nature, the

"meanest and most ill-formed of creatures that chew the cud."

Remarking on the sluggardy of the sloth, Cuvier (1837) explains it is "because of a construction truly heteroclite." Nature, he says, seems to have amused herself by producing something imperfect and grotesque.

Some recent engaging articles on sloths have been written by Beebe (1925, 1926). Observing that it lives at a low ebb, or in low gear, the personification of deliberation, he adds that the sloth would be a fitting inhabitant of Mars, where the year is six hundred days long; self-centered and sub-reptilian in mentality, in a "perpetual estivation of sorts," it had no right to be living on earth. Probably about the lowest in its scale, as to touch, taste, sight and hearing, it was nevertheless better off, olfactorily speaking, than man. Many interesting habits and activities of sloths are described by Beebe, and several anatomical facts are given. Jaguars and wild cats, coatis, the anaconda and the harpy eagle are listed as enemies which at least occasionally eat sloths.

In a number of recent contributions, Wislocki has dealt with the important anatomical aspects of sloths; these are referred to in detail later. Extended bibliographies are given by this writer, and also by Beebe (1926). Other articles in the literature on the sloth are appropriately referred to herein, but no attempt has been made to give a complete bibliography. Most of the data contained in this review are, it may be said, derived from the writer's own records of experimental work carried out during several expeditions to the tropics. In nearly all cases observations were made on the commoner Panamanian sloths, the two-toed *Cholepus hoffmanni* Peters and the three-toed *Bradypus griseus griseus* Gray.

Three expeditions to Panama were made by members of the Department of Physiology of the University of Virginia Medical School in 1937, 1938 and 1939. Members of these expeditions besides the author were Mrs. S. W. Britton, Dr. Murray Brown, Mr. W. E. Atkinson and Mr. Raymond F. Kline. To these workers credit is due for much of the information contained herein, as indicated in separate scientific reports under their authorship. The author worked under the terms of a John Simon Guggenheim Memorial Fellowship during the years

FIG. 3. AN IMAGINATIVE, PLAYFUL SCENE OF YOUNG SLOTHS PICTURED BY BUFFON (1776)

1937 and 1938. Work was carried out in the first two years chiefly at the Institute for Research in Tropical America at Barro Colorado Island and surrounding territory, with the splendid help of Mr. James Zetek, Curator. Here it was possible to live with large colonies (50 to 100) of sloths, imported from the mainland, and observe very intimately their living habits. In 1939 investigations were undertaken at the Gorgas Memorial Laboratory, and from there field trips were made, with the invaluable co-operation of Dr. Herbert C. Clark, Director of the Labora-

tory. The author is much indebted to Mrs. Britton for the drawings contained herein.

CLASSIFICATION AND ANTIQUITY

Sloths may be designated briefly, zoologically, as of the mammalian subclass Eutheria, order Edentata, sub-order Xenarthra, and family Bradypodidae. In the two genera, *Bradypus* and *Cholepus*, several species have been described—*Bradypus infuscatus*, *B. griseus griseus*, *B. tridactylus*, *B. castaneiceps*, *B. ignavus*, *B. torquatus*, etc.; *Cholepus hoffmanni*, *C. didactylus*, etc. Differences in species appear to be highly variable and in considerable dispute, however, especially in the case of *Bradypus*. Four other distinct families also occur in the Edentates besides the Bradypodidae, namely the Manidae, Dasypodidae, Myrmecophagidae and Orycteropodidae. It should be said that the Edentates have been divided by some workers into the sub-orders Pilosa (including the sloths) and Loricata (armored Edentates).

Flower (1882) states that probably all the New-World Edentates, which represent by far the majority of the known species, sprang from one common stock in the Pliocene and Pleistocene periods, relatively late in geological history. In contrast, in his work on placentation in the sloth, Wislocki (1927) remarks on the possibility of the great antiquity of the Xenarthra, including the sloths. The work of the present author also indicates that the sloth is a relic of a dim and ancient past. Some interesting connections of surviving sloths with the gigantic extinct Tardigrada (Glyptodontidae, Megatheridae) have been suggested by some workers. According to Miller (1939), however, there is little or no conclusive evidence regarding the extinct forerunners of present-day sloths.

The term "sloth" was used to denote the animals described herein more than 300 years ago. Purchas (1613) writes that "The Spaniards call it . . . the light dog. The Portugals Sloth, The Indians, Hay." A further early reference is that of Crew (1681), who mentions "The Sloath. . . . An animal of so slow a motion, that he will be three or four days, at least, in climbing up and coming down a Tree." The two-toed sloth, *Cholepus*, possessing two toes on the fore-limb and three on the hind, also is given the common but still little-known name "unau," of South American (probably Tupian Indian) origin. *Bradypus*, or the three-toed sloth, having three toes on both fore and hind feet, is known commonly by the name "ai," an onomatopoeic term having reference to the shrill, bleating cry occasionally emitted by the animal, especially at night or when molested.

The delightful early Spanish term of ironical concept for a sloth is *perico ligero*, or nimble little Peter! By far the slowest and most typical of sloth-like animals, the antithesis of speed, are the three-toed Bradypodidae. The Cholepine two-toed sloths are in contrast much better grade or higher type animals; this is apparent from many physiological aspects, although they are still distinctly and properly classified as sloths. Some of the slowly-moving lemurs would almost certainly show functional correlations with the sloths, but zoologically they are widely separated from *Bradypus* and *Cholepus*. The sloth bear, sloth monkey, ground sloth and sloth animalcule are interesting extinct or present-day animals which are not included in the subject of this study.

Distribution

Sloths are found in the forest regions of tropical America from Honduras and Nicaragua in the north down through Panama and as far south as upper Brazil and Bolivia. They occur on both sides of the

Andes, a fact which earlier observers did not recognize. They are strictly limited to the New World, it may be emphasized, although several of the older historians indicate otherwise. It is nevertheless true that other animals inhabiting the Old World, such as African potto-lemurs and Asiatic lorises, have in the literature been popularly called sloths.

Bradypus occurs only in the lower neotropical altitudes, and is particularly prevalent in regions where the temperature is not widely fluctuating (in the heavily-wooded jungle depths). *Cholepus* also lives chiefly in the warm, tree-shaded lowlands, but is able to withstand the cold (possibly occasional freezing) of altitudes up to 7000 or 8000 feet. The latter form apparently does not extend farther north, however, than Costa Rica. Most species of *Bradypus* are distributed generally throughout the neotropics, although *B. castaneiceps* and *B. ignavus* have a more northerly distribution, and are particularly common in Panama and Costa Rica. *C. hoffmanni* is also commonly found in the latter places, as well as in Ecuador. *C. didactylus* inhabits chiefly Brazil and the Guianas. The largest sloths observed by the author have been those taken in Colombia. It appears likely from many considerations that these animals thrive best in the highly humid and wooded equatorial regions.

Characteristics

General

In both genera of sloths and especially in *Bradypus* the head appears small for the body size. The broader snout and head in *Cholepus* are due in large measure to the extensive sinuses which are present. The external ears are very small and hidden in the hair; in *Bradypus* only a tiny pinna about 1 cm. across is present. The eyes are also small and round, and of a dirty brown color. Passage through trees is obviously facilitated by the head arrangements.

Contrasted with the short neck of *Cholepus*, that of *Bradypus* is long and mobile, and along with the small head is suggestively reptilian. It is remarkable that the Bradypodine head may be rotated through 360°; starting from the dorsal facing position a complete turn may be made. The fore-limbs in sloths are longer than the hind, which appears important in meeting the demands of arboreal life. In *Bradypus* the fore-limbs are longer, the hind-limbs shorter than the respective limbs in *Cholepus*; there is also greater mobility of the fore-limbs, which may indeed be drawn around to the back of the body, crossed, and returned forward to meet again over the abdomen. The hind-limbs of sloths (greatly shortened in *Bradypus*) serve a great deal of the time for maintaining hold or stance. A stumpy tail 8 or 10 cms. in length is present in *Bradypus*, while *Cholepus* is tailless.

Cholepus is much the larger, stockier-built animal, and is considerably less sluggish—under ordinary conditions it travels about twice as fast as *Bradypus*. Adult didactyl animals reach weights between 6.5 and 7.5 kgs., while the tridactyl form weighs between 3.5 and 4.5 kgs. The females are smaller by about 0.5 kg.

About as large as a small or medium-sized dog, therefore, sloths are markedly different in general form and appearance. The shaggy coat, long limbs and long curved claws, small head with bead-like eyes, and remarkably slow movement in the upside down position, set the animal apart from all other types. A very furry or hairy "teddy bear" indicates somewhat its general appearance (Figs. 4, 5, 9, 10), the young sloth especially resembling a ball of rather fluffy fur. For several months after birth the young possesses a

FIG. 4. THE TWO-TOED SLOTH (CHOLEPUS HOFFMANNI) IN VARIOUS POSITIONS

1 and *2*, fighting; *3* and *4*, nursing its young; *5*, normal walking; *6*, a bath before immigration check-up, on going north from Panama.

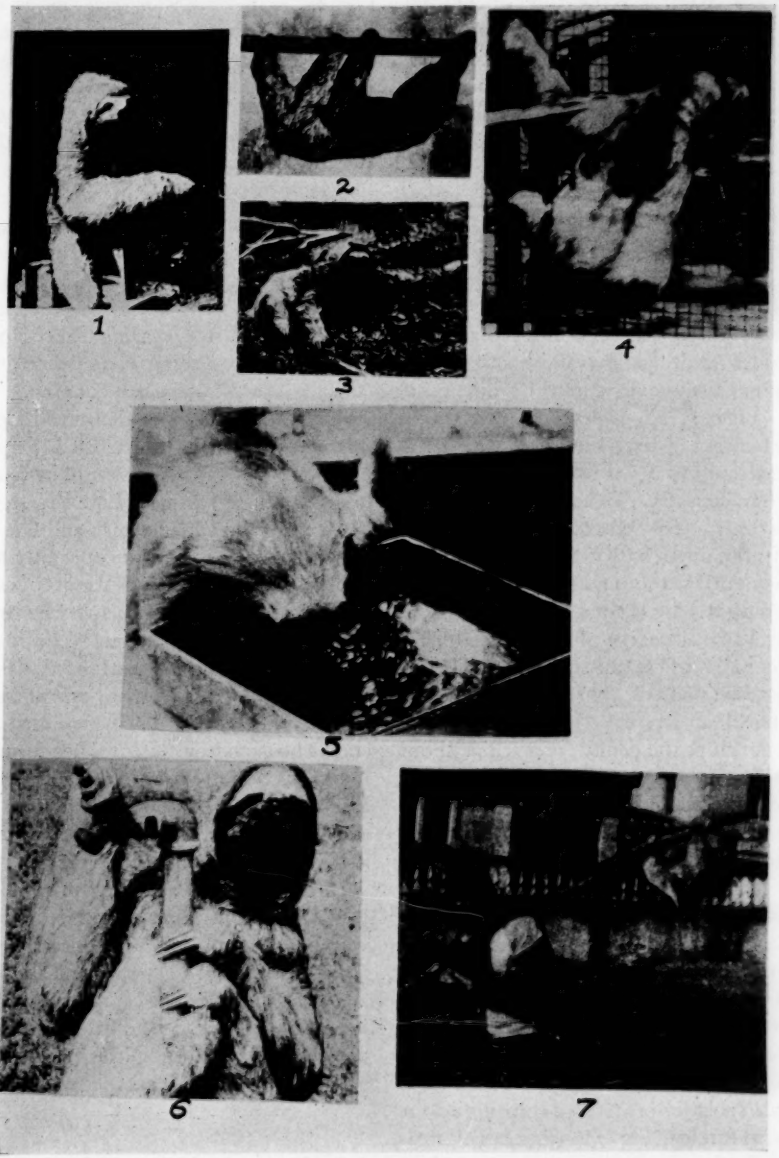

FIG. 5. *1, 2, 3* AND *4*. THE THREE-TOED SLOTH IN TYPICAL POSITIONS, THE LAST-NAMED (*4*) A MALE WITH DORSAL SADDLE MARK; *5* AND *6*. TWO WAYS OF DRINKING IN THE LABORATORY; 7. NATIVE BRINGING SLOTH IN FROM THE JUNGLE

soft, fine coat, and the three-toed form displays fur as an undercoat intermingled with coarse hair throughout life.

Bradypus is found, when shorn of its outer hair, to be beautifully marked with a black and white coat, somewhat leopard-like in character but with coarse linear mottlings of glossy black from 1 to 3 cms. wide. Both male and female three-toed sloths are thus observed to be marked when clipped. The male tridactyl animal gives a clue to this condition by its dorsal exposed white and black mottled area (in the older adult, yellow and black), some 10 by 15 cms. in extent, in the saddle position (Figs. 5, *4*). This very distinctive dorsal marking has received no explanation, except to say that it represents a male sex characteristic. It begins to appear in males only a few weeks of age, and evidently the outer hair is then lost by slow degrees until the animal reaches about one year old, at which time it seems as if the area had been neatly plucked or clipped. Wislocki (1928) states that the beginning of a saddle mark is apparent in fetal male and female Bradypods, but that the male only develops the definitive mark at about the time of puberty. The condition may be reproduced in the female by shearing (Fig. 6, *1*). In the two-toed sloth both under and outer hairy coats are similar in color and texture, and in this form there is no sex marking (Fig. 6, *3a*).

Male and female sloths are difficult to distinguish, except for the dorsal marking in *Bradypus*. After long practice, slight differences in the urogenital area, indicative of the sex, may be detected.

In both genera, the long, coarse hair affords splendid protective coloring in the tropical foliage (Fig. 7). Patches or areas of different shades are very common. Various tints of gray are observed in *Bradypus*, while gray-brown (sometimes greenish-gray) usually predominates in *Cholepus*. In the latter the coat is much less shaggy, and in its best condition it is indeed of a rather sleek, smooth appearance. The hair in *Cholepus* inclines back from the head and limbs and abdominal areas towards the mid-dorsal line and tail. In *Bradypus* also the hair exhibits this tendency, but is much more irregularly disposed and shows a frontal ruff, a central dorsal division and small shoulder and pelvic whorls (Fig. 8). In the case of both genera the hair arrangement undoubtedly allows optimal shedding of water, which is highly desirable in the (usually) very rainy sloth country.

In the didactyl sloth the hair under the microscope is found to be fluted longitudinally—the only known case of such mammalian hair markings (Fig. 11, *A*, *A1*). Exceptions are the vibrissae and fine hairs about the eyes, which are normally round and plain. From 8 to 10 grooves and columns, well rounded and the former with a tendency toward lateral hollowing-out, are found in cross-section in each hair; they run from base to tip, where all disappear uniformly. The three-toed sloth shows no such hair flutings, but transverse markings occur (Fig. 11, *B*, *B1*). In both forms the hair is scaly. Individual hairs appear under the microscope as white, yellowish, brown, gray, or black.

Different coat shades and ruff markings, and also skull contours and number of vertebrae, have influenced observers in designating "new" species. These characteristics are, however, highly variable in any small group of sloths collected in one locality, and the author sees no good basis for many of the species described.

Activities and habits

In the same order—the Edentates—with the aard-vark, pangolin and other divertingly interesting forms, the sloth presents an equally engaging cross-word puzzle to

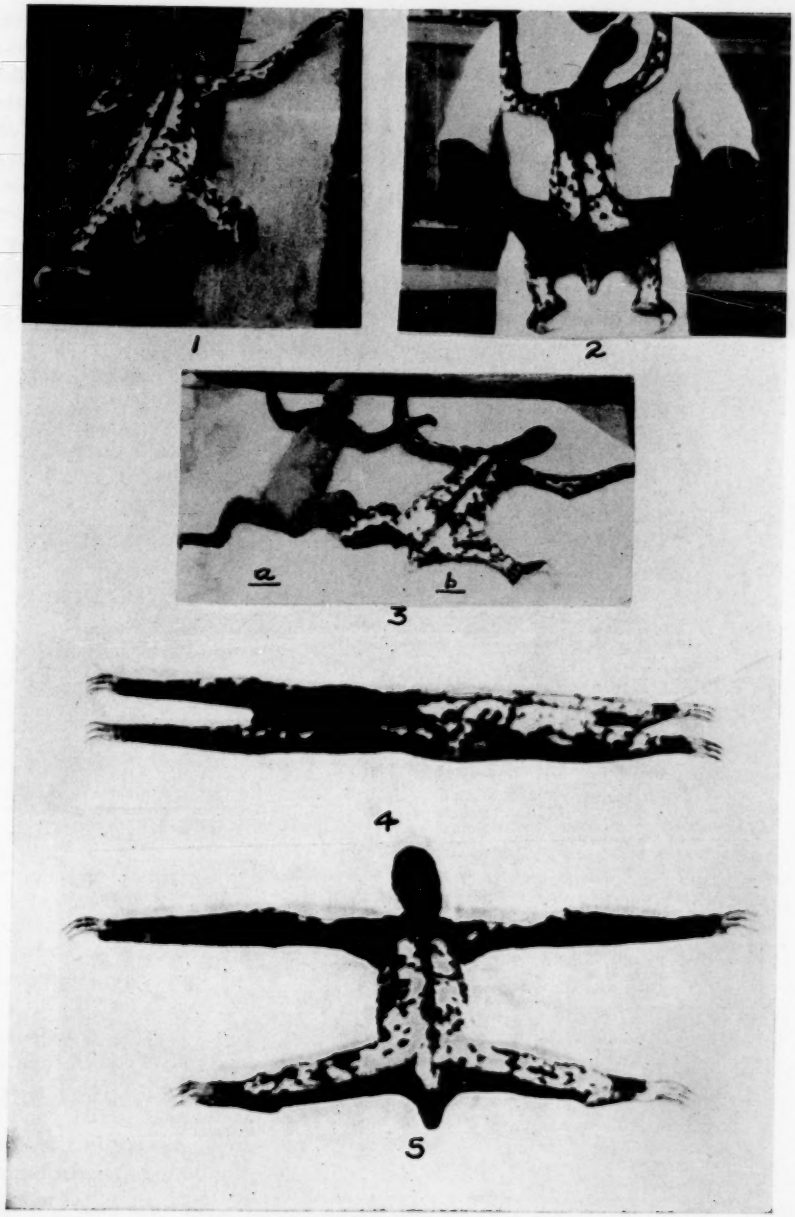

FIG. 6. MALE AND FEMALE SLOTHS SHORN OF OUTER HAIRY COAT, EXPOSING UNDER-FUR

All are three-toed animals, except *3a*, which is two-toed and of a uniform gray color. The black and white leopard-like mottlings in three-toed sloths are well seen in *1* and *2*; in *4*, the extraordinary streamlined form is observed, and in *5*, the very long, slender limbs and extremely long claws are evident.

both naturalist and experimental scientist. "One more defect," wrote Buffon, "and it would cease to survive." But, while it is a defective creature, comments Beebe (1926), and a sloth in Paris would surely not long survive the pace, Buffon clinging upside-down to the branch of a tree in the jungle would expire even sooner! Spending most of its life in the mid-jungle trees, looking much like a hanging mass of gray-beard mosses, the sloth is wonderfully built for clinging and climbing. Normally, it travels on the under side of limbs in the inverted or back-down position, and shows very poor ability to progress in the upright position. While it usually advances head-forward, to be sure, the three-toed sloth may frequently pivot and go tail-first, while maintaining the same direction of movement, say along a branch or pole; and it may thus change about several times while traveling, in the course of a minute or two. Tail-forward movements are as fast as, or no slower than, those made head-foremost. Again, there is frequently no definite order of limb-movement maintained by tridactyl sloths, and one gets the impression that a "touch-and-go" style of progression is followed. In such an empirical way of travel, one movement is probably not dependent on or conditioned by that immediately preceding it so much as by the specific position of the body in space, which is modified from moment to moment by the swaying branches along which the animal moves. Another influential factor is, of course, the proximity of food.

FIG. 7. THE SLOTH (THREE-TOED) IN A TYPICAL CRAWLING POSITION ON THE GROUND, AMONGST SOME CULTIVATED PANAMANIAN GRASSES

In the jungle trees and grasses the shaggy, patchy-colored hair is an even more effectual protective covering against detection.

Sloths thus appear to possess an extremely limited sense of equilibrium. The upright position on all fours is seldom assumed under normal, active conditions, when the animal perches in tree crotches or comes to the ground. In the latter case progression is very slow (even for the sloth) and difficult; the spread-out or sprawling position of the body and limbs

indicates an undeveloped musculature (for this situation), and also demands no effort in balance. Although its home for the greater part has been made in the trees, the sloth has resisted to only a slight extent the influence of gravity, and covets the delights of dangling and swinging in space. With its remarkably long, curved claws, it is also well adapted for maintaining a fixed position with ease. This it appears to do most of the time, if one may credit the observations of hunters and laboratory workers.

In justice to the sloth, however, its normal mode of progression gives it easier and surer access to pendant fruits and leaves on which it feeds. Only a slight development of attitudinal and equilibrational reflexes would appear to have been demanded. In the case of monkeys, the

FIG. 8. YOUNG THREE-TOED MALE SLOTH, CLINGING TO VERTICAL WALL
The hair lines of limbs and back are typical, and the dorsal saddle mark is about one-half developed in this case

usual upright or head-up position involves, in contrast, a much more complicated set of reactions; and while these primates may occasionally be observed to slip or even fall from the trees on which they are feeding, the sloth is in contrast the surer-footed and more specifically adapted animal for arboreal life.

FIG. 9. SKETCH OF THE TWO-TOED SLOTH (CHOLEPUS HOFFMANNI) TAKING UP TYPICAL FIGHTING POSTURE WITH ARM RAISED TO STRIKE

FIG. 10. LIFE-SKETCH OF THREE-TOED SLOTH (BRADYPUS GRISEUS) CARRYING YOUNG ON BREAST, AMONGST CECROPIA FOLIAGE, ITS COMMON FOOD

Bradypus is a great sitter; the greater part of its life, perhaps four-fifths, appears to be spent thus, sleeping or dozing in the branches or smaller forks of trees, with fore-limbs often crossed over the breast and head tucked upon them. Notwithstanding this, and the fact that the sloth's stomach is always chock-full, it never accumulates any fat or "puts on weight"! The higher trees are sought for rest when the foliage is thin, and the lower ones when the vegetation is dense enough for hiding. The stumpy tail helps considerably as a prop in providing the personal sitting quarters, and this is possibly correlated with the stunted and worn-down appearance of the caudal member.

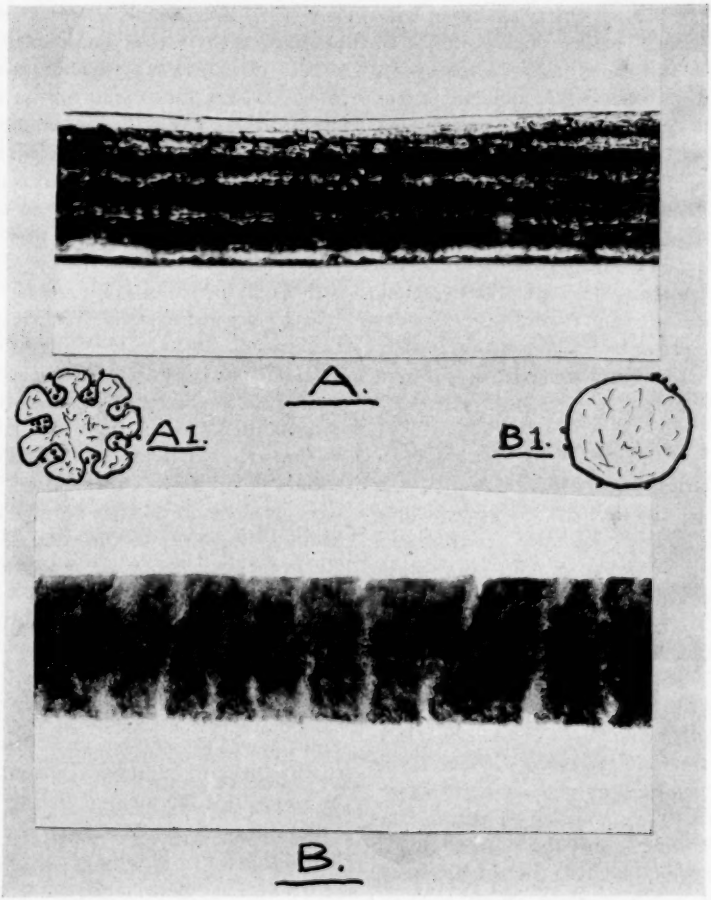

FIG. 11. MICROPHOTOGRAPHS OF TWO-TOED (*A*) AND THREE-TOED (*B*) SLOTH HAIR
A1 and *B1* indicate the respective hair shapes in cross-section. The flutings in the Cholepine hair are unique amongst mammals. The great numbers of algae infesting the hair are evident.

Cholepus also spends a great deal of time

in the squatting posture, but rests occasionally in the upside-down position, suspended by all four limbs. Wislocki (1928) has stated his belief that *Cholepus* spends much of its time in the latter position, but the present writer could not confirm the point. In observations made in Panama on a large colony of sloths over a period of two weeks, at different times during the day, the two-toed variety was found when resting to maintain the upright clinging or squatting position in 85 per cent of the counts, and the three-toed animal in 90 per cent (Britton, unpublished observations). At other times the inverted, clinging posture was assumed. The rather unnatural conditions present in even a jungle laboratory would modify to some extent, however, the deportment of the sloth.

Habitat and travel

Commonly a particular neighborhood is favored by individual sloths for long periods at a time. In some cases an animal has been observed to remain even in the same tree for days or weeks. Favorite roosts in trees and thick foliage have often been observed. The alcabu or prickly yellow tree, possessing large spines on its trunk which defy climbers, appears to be a common resting place of *Cholepus*. Excursions for food are made particularly at night, when both species are most active. The Indians have a saying, however, that when the wind blows the sloth begins to travel—and in much of the sloth country the trade winds start at about 10 A.M.!

Movement is made from tree to tree via the ends of branches when possible (as it usually is in the thick jungle), but descent to the ground for passage may occasionally be made. While the more agile two-toed form is rarely seen on terra firma, *Bradypus* may be met fairly often on the highway or trail, and it sometimes approaches the outskirts of towns. Mating and parturition apparently occur on the ground in both species (see later).

Sometimes, distances of several miles may be covered in the course of a few days. The sloth is moreover a good swimmer, and may cross rivers or a mile or so of open water. The latter fact has been observed in the Gatun Lake area of the Panama Canal, where sloths fled from the islands about twenty-five years ago when the lake was formed. Today, these animals baffle all attempts to insulate them in this area by swimming to the mainland whenever they are released on even the larger islands, such as Barro Colorado.

There are indications that considerable numbers of sloths may move or migrate from one locality to another, several miles distant, in the course of a few years. Such changes do not appear to be related to food or climatic conditions, or to the larger animal enemies. Observations which have been made on this point have been limited, however, to the central parts of Panama, particularly to the Canal Zone where man has extended his activities rather rapidly, and they may possibly not hold for other areas.

In the jungle, sloths are usually found resting or travelling as solitary individuals. Many natives assert that if one sloth is discovered, however, another may be found not far away. In one very unusual instance, native workers near Cristobal sighted five three-toed sloths in different parts of one large tree. Both didactyl and tridactyl sloths may range over the same neighborhood together, while in other cases one genus may occupy an extensive area alone. In the case of long-confined animals under laboratory conditions, each genus may be observed congregating separately, in the heat of the day, in shady corners of the pen.

Males and females associate together in this way indiscriminately.

When first taken and placed in open-air cages near the laboratory, the sloth appears ill at ease and continually seeks a way of escape. Clawing of the cage and frequent sniffing of everything animate and inanimate within range are observed over a period of three or four days, after which time the animal settles down to an apparently normal, sedentary life.

Fighting propensity

When sloth meets sloth there is usually a fight, at least in the rather restricted quarters of the laboratory. Two-toed sloths are aggressive, and when approached will readily tackle almost any animal including man. *Bradypus*, however, is a poor fighter and never appears to pick a quarrel, but will strike back weakly when molested.

The fighting *Cholepus* usually holds on grimly by its hind-limbs to a branch or the cage wall, and lashes out fairly quickly with one or both long and dangerous forelimbs, while teeth are bared and the powerful jaws stand open ready to come together on any object with resounding clack (Fig. 4, *1*, *2*). An attempt is made to draw an adversary, when caught, towards the open jaws. The claws and teeth are formidable weapons, and even sloths themselves with their supertough hide may be badly scarred and torn in combat with each other. An inch pine board may be bitten through and splintered readily. The fore-limb flexion or striking movement of *Cholepus* is fairly quick, and has been called a "pugilistic short-arm hook" (Beebe, 1926); the preparatory extension component is, however, a slow, "winding-up" affair.

Tenacity to life

Wounds in both forms usually show clean, rapid healing, but deep injuries may become badly infected. An amazing tenacity to life is shown by sloths in their ability to survive for long periods after extensive trauma, apparent drowning, or anesthetization far beyond respiratory failure. Beebe (1926) has mentioned for example that a sloth may survive immersion for 40 minutes in water—almost as long a period as the whale may spend submerged! In the writer's experience, recovery of an animal may also occur after suppression of respiration for 30 minutes by ether.

In some tropical areas the sloth has been used by natives for food, but the flesh is very tough and most difficult to strip of its hide. In other parts there are definite taboos, or "trafers" against killing the animal. Kahn (1931) remarks that individual natives may have particular trafers: thus, one may not shoot a sloth in Dutch Guiana, but if he should do so, his gun would be spoiled. Amongst native hunters it is rather generally considered to be unlucky to shoot or otherwise injure the sloth.

If one may make a rough approximation regarding age from observations on about 300 sloths in their native country, it would seem likely that these animals have a normal life-span of from 8 to 12 years, and may perhaps even live up to 15 years of age under optimal conditions.

Ability to learn

Although it puts up a weak resistance to capture in the jungle, *Bradypus* soon becomes docile and shrinking in the surroundings of the laboratory or garden, and may quickly become a pet. Colombian and other natives may now and then be seen travelling with such animals as pets clinging to their backs. *Cholepus* is tractable when young, but rather difficult to tame in adulthood. When kept in the laboratory for some weeks, however, it

tends to become fairly sociable. The earlier American workers on the Panama Canal collected sloths, among many other interesting forms, and hunters and others there today occasionally take the animals home to their gardens as attractions.

Some ability to learn or develop certain habits is shown, especially by *Cholepus*, after a week or two under laboratory conditions. Regular eating and drinking habits become established readily (Fig. 5, *5*, *6*), and food such as a banana may be eagerly sought after, and even delicately taken from one's hand. The appearance of the water boy signalizes drinking time, and turning on a faucet in the pen quickly attracts the thirsty individual. *Cholepus* has been observed to drink from a running water tap.

Fairly good responses are noted in training the sloth to travel on a horizontal bar or along a special track; also, when one makes frequent blood samplings from a vein, a certain adaptation of the sloth to such treatment is observed. Individual haunts in the pen are recognized and honored after animals have been in captivity for a few days. In the tropics both genera may be kept indefinitely under good conditions in the laboratory. In cooler latitudes also *Cholepus* lives well, but *Bradypus* is kept alive only by very careful diet and attention to environmental temperature.

Sloth parasites

Sloths afford harbor for an assortment of animal and even some plant life. The hair of both *Cholepus* and *Bradypus* is thickly covered with small green algae similar to the common parasite *Chlorococcum* (Fig. 11): *Bradypus* usually shows the greater infestation. Young sloths acquire the parasitic covering after they are a few weeks old. The algae are very densely packed, and occur irregularly or in columns or chains, one to four in number wide, around the hair, in the case of the three-toed sloth with ordinary round hairs. Similar chains are found along the hair length, but in the grooves, in the two-toed animal with fluted hair. The dorsal parts are particularly well covered with algae. A rather bright green coloration is thus developed over the more exposed areas of the sloth, because of the algae infestation, in the wet season of the year. In the drier periods the algous covering tends toward a brownish-green hue, although a little water sprayed over the animal restores the brighter green tint.

Supporting this protective coloration afforded the sloth in its changing jungle environment may be noted the coarse, moss-like hair itself, strikingly similar to the surrounding masses of epiphytic vegetation. When these facts are coupled with the animal's very slow movements and tendency to secrete itself, sleeping hours on end, in the thicker foliage or angular, more inaccessible branches, the preservation coefficient of the sloth would appear to have been set at a high level. With such relatively passive or easily acquired and effective survival factors, the limited defensive ability of this tardigrade (especially *Bradypus*) and also its uniparous habit are in good biological harmony.

The sloth pelage, besides being covered with small algae, gives gracious refuge to several higher forms. Moths are at home in the hairy coat, small beetles have been observed, and ticks burrow through and into the skin to find their living. In the tropical laboratory, large cockroaches sometimes find a hiding-place and possibly food in the sloth hair. With rather generalized internal vermic infestations added to the list, the sloth takes first rank as a "buggy" animal. Several species of moths, ticks, and roundworms which infest the sloth have been described. A few

generally interesting facts on some of these parasites are given here.

Sloth moths are the only Lepidoptera known to be ectoparasitic in the adult stage. Three which have been described are of the family Pyralidae, and belong to distinct genera. The moths show great activity, scurrying about in the deep hair, as soon as the sloth is caught; if much disturbed they will fly off to a nearby perch, or settle on the intruder's hair, and then return to their host later. They do not appear to spend all their life in sloth hair, however, since they have been taken in night-traps. Further, they do not seem to feed while on the sloth, since the mouth parts of all specimens examined have appeared coiled and atrophic. The larvae have apparently not yet been recognized but are possibly present, and may feed on fats on the sloth hair or on the abundant algae.

Four species of ticks have been described as infesting *Bradypus*, and apparently one or two species are found on *Cholepus*. Although sloths occasionally scratch and preen themselves, very little attention appears to be paid to the ticks, which are found on different parts of the body and sometimes attain a large size, some 2 cms. or so in diameter.

A small black insect, apparently a beetle and about 3 mms. in length, has been observed occasionally by Beebe (1938) and by the author in the commodious sloth hair. It does not appear to have been described in the literature.

Three or four different helminth parasites (Filariidae, Spiruridae, etc.) have been observed in sloths (Fig. 12). They occur in the stomach, mesentery, and other parts, sometimes in great numbers, and many appear to be peculiar only to the sloth. Again, the three-toed form would appear to be the greater sufferer; at least, most vermes have been observed in this animal.

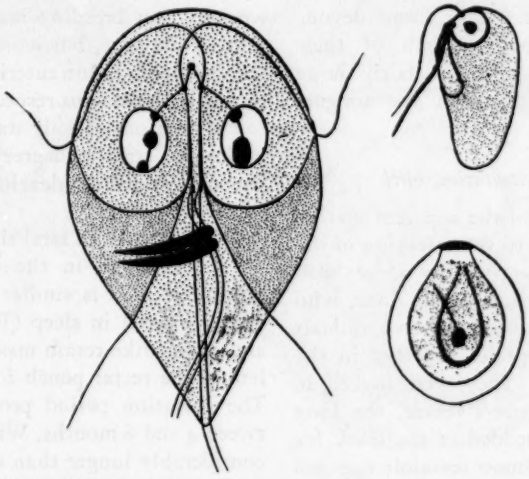

FIG. 12. SOME INTERESTING PARASITES FROM THE SLOTH INTESTINE (FROM HEGNER AND SCHUMAKER, 1928)

Food

Voracious eaters under natural conditions, sloths apparently utilize many varieties of leaves and fruits for food. When

an animal is taken in the field, the stomach contents may be found to comprise more than a quarter of the body weight. The gastric sac is really amazingly large. Even under laboratory conditions, didactyl sloths consume large amounts of foodstuffs—bananas, oranges, figs, lettuce, hibiscus, and especially cecropia leaves. Tridactyl animals in captivity may usually be coaxed to take cecropia and hibiscus leaves, but sometimes refuse all food and succumb within a week or so. Raw beef was taken from the hand and eaten fairly readily by *Cholepus*, while *Bradypus* showed little interest in it. Both forms devour the placenta following birth of their young. Water is taken in fairly large amounts by lapping with the tongue, similar to a dog.

Mating, placentation, birth

Only one report of the apparent mating of sloths has come to the attention of the author. This was given by three bush workers in the Panama Canal Zone, who stated that they came across two animals of the two-toed species together in the tall jungle grass. They were locked in close embrace, ventre-à-ventre, the long claws of each embedded in the thick fur of the other, and almost certainly engaged in sexual intercourse, according to the witnesses. No signs of fighting were evident. Several general muscular spasms were said to have taken place over the period of about half-an-hour while the animals were observed. It may be said that although the sloth usually will scuttle away as fast as it can travel on the approach of man, not the least attention was paid in this case to the onlookers. It was found impossible to separate the the animals by force, and they were left to their own devices by the usually kind natives. The ventre-à-ventre position in mating would appear to be demanded by the anterior position of the genitalia and the forward-tilting pelvis.

Some cryptic comments have been made by Beebe (1926) on the mating of sloths, which he notes occurs in spring. The courtship, he relates further, is "unemotional, direct and brief."

Following the older work of Turner (1873), a number of observations have been made in recent years on placentation and the early development of sloths. Those of Wislocki (1927) and Heuser and Wislocki (1935) are very interesting and comprehensive. It is indicated by these workers that breeding may occur at any time of the year, but more often early in the dry season. Intrauterine development showed in numerous respects, particularly in the relations of body stalk, amnion and yolk sac, a striking degree of similarity to the corresponding developmental stages in man.

The hair lines in fetal sloths are said to be the same as in the adult, and the posture *in utero* is similar to that of the grown animal in sleep (Fig. 13). Fetus and adult alike retain masses of fecal pellets in the rectal pouch for long periods. The gestation period probably lasts between 4 and 6 months, Wislocki states, or considerably longer than that of cats and dogs.

Several sloths have been born in the laboratories set up by the author and his associates, both in Panama and in the more natural surroundings at Barro Colorado Island. Only in two cases, however, have the birth processes been observed from the beginning. These are recorded below.

Gorgas Memorial Laboratory, March 17, 1939.

At 1.20 p.m., the attention of Tom, the laboratory boy, was drawn to one corner of the pen, where a two-toed sloth was crouching and being teased by a group of chattering monkeys picking at it from the adjoining cage. The sloth made low groaning sounds occasionally, as it apparently experienced labor pains. It frequently stretched itself, and then drew

up into a ball-like mass. Presently a small amount of blood was noted about the urogenital opening. No other fluid appeared.

At 1.30, the top of the head of the baby sloth was observed at the opening, and ten minutes or so later the shoulders appeared.

At 1.45, the baby sloth was expelled fully enclosed in a rather thick, loose-fitting periderm or epitrichial membrane. The membrane invested all parts of the body like a garment and was not of the usual mammalian, amniotic sac-like formation. The umbilical cord was attached. Within a few moments the placenta appeared. After parturition, the mother quickly began to claw and chew the membrane, and the new-born sloth struggling vigorously at the same time caused its coverings to rupture further. A little later the mother chewed up the placenta, and began to swallow it in small fragments. The new-born sloth meanwhile began to crawl about on the mother's fur, and in the near vicinity. Occasionally, tension on the cord held with the placenta by the mother caused the young one to cry out with a long, high-pitched squeal. Every little while the mother licked and fondled the baby, and also ate remaining fragments of the periderm. The mouth parts of the mother were covered with blood at this time.

At 2.05, the baby was very active. It was withdrawn from the mother, upon which the latter displayed resistance and anxious concern. Several photographs were taken.

At 2.40, when the young one was returned, the mother again became very excited, and then showed considerable affection for the offspring. In a few minutes she again began eating the placenta, and soon had devoured it completely; later the cord was bitten off close to the baby's abdomen. The mother ate part of a banana, but was evidently

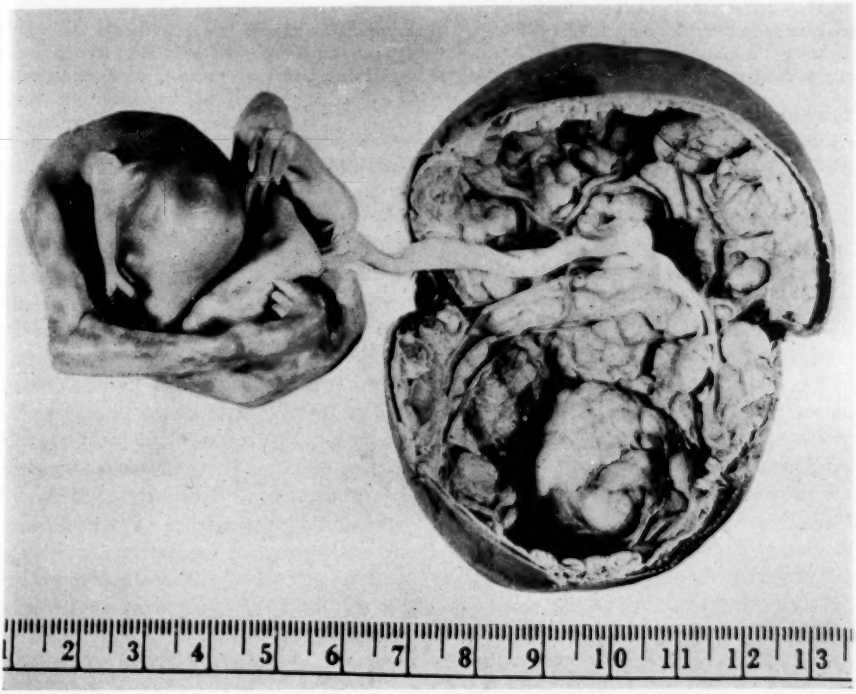

FIG. 13. FETUS OF THREE-TOED SLOTH AT ABOUT TWO-THIRDS TERM, WITH CORD AND DIFFUSE PLACENTA ATTACHED

The hair has not erupted, and the epitrichial covering of the fetus has therefore not lifted. Note the small pinna and the extremely long fore-limb (6 cms.) compared to a total body length of 10 cms. The tail is relatively prominent at this age.

fatigued and soon fell asleep with its baby on its breast.

At 3.30, the weight of the young sloth was found to be 340 grams, and of the mother 3.97 kilograms.

The mother sloth became much disturbed every time its newborn was temporarily removed, running (sloth-like!) around the cage, clawing and biting at all objects and growling viciously. When given its young one again it immediately became quiet, and began licking and nursing it with great care. It also cleaned all its accessible parts by licking, giving special attention to the urogenital area.

Next day the mother sloth still retained the same corner of the large cage, carefully nursing its baby.

On another occasion (May 10, 1937) a two-toed sloth was born dead in the laboratory. It was enclosed in a thick epitrichial sheath, but no placenta was observed at the time. The young sloth weighed 398 grams.

The birth of one three-toed sloth has been observed. When the attendant noticed the mother, it was in one part of the large cage alone, stretching and moving occasionally as if in weak labor. The top of the oncoming head had appeared at the urogenital opening; there was no evidence of blood or other fluid in the neighborhood. Within three minutes the young sloth was fully delivered, covered completely with the epitrichium. It struggled and cried, and the attendant slit the membrane and peeled it away, and also severed the umbilical cord and tied it. The remaining end of the cord was left extending from the urogenital canal. At no time during these procedures did the adult tridactyl animal show any maternal concern for its offspring. The newborn sloth, when placed near its mother, crawled quickly on to the latter's breast and began nursing. Later the mother was found to weigh 3.62 kilograms, and the young sloth 250 grams.

No placenta or cord was found in the pen some time afterwards.

The young one was kept with its mother for some time under laboratory conditions, and both did well.

(To be concluded)

THE SEX RATIO, FERTILITY, AND ANCESTRAL LONGEVITY

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THE proportion of the sexes within the family, tribe, or nation has long attracted man's attention. The fundamental desire for survival would alone have made the sex ratio an important question for reasons economic and martial. Added to this is the closely related wish to extend survival into the future through lineal descent. Some of the earliest writings indicate that such descent was believed to be through the males of the group, and fathers have, therefore, always put a premium on male offspring. It is no wonder that attempts to alter the proportion of sexes should have first found expression in theories concerning sex determination, and recipes for producing the desired sex at will. With the lack of knowledge concerning the anatomy and physiology of the reproductive system, ancient theories were purely products of speculation.

EARLY THEORIES OF SEX DETERMINATION

Hippocrates (460–370 B.C.?) [translated by Adams, 1849] believed that the male foetus was located in the right side of the uterus, and the female in the left. This was based upon the idea that the human uterus was bipartite. The theory was enlarged upon by Aristotle (384–322 B.C.) [English edition by Moritz and Chambers] who prescribed a course of procedure for begetting children of the desired sex. The woman wanting a male child should lie on her right side after intercourse, in order that this side might be the place of conception, "for therein is the greatest generative heat, which is the chief procuring cause of male children." The method was said to fail only rarely, especially if the woman kept warm and with little motion, also drinking hissop and saffron in a glass of malaga. In order to have a female child the woman should lie on the left side and think strongly of a female, for Aristotle believed that imagination of the mother might often determine the sex. Galen (130–200 A.D.?) [translated by Daremberg, 1856] also thought that male offspring were formed as a result of greater heat. Not only did he believe that the male was formed in the right side of the uterus and the female in the left, but also that the right and left testicles of the father formed male and female children respectively. The difference in heat of the two sides was supposedly due to greater vascularity on the right.

During Aristotle's time there were several other ideas concerning sex determination. He said that the best time for a conception to result in a female birth is in the wane of the moon while in Libra or Aquarius. He also mentioned the ancient physicians and philosophers as having said that the sex of the child is decided by the relative strengths of the

male seed and of the menstrual blood, but he thought this to be a fallacious theory.

The writings of Hippocrates, Aristotle, and Galen dominated thought for many centuries. Lemnius (1572) wrote what was representative of the knowledge of his time when he considered the uterus as a plowed field for the male seed to grow on. Whether the child would be male or female depended upon which side of the uterus the seed fell, the temperature, the time of year, and the amount of menstruation. This, it will be noted, is identical with views expressed almost two thousand years earlier. Some of these ideas have in some degree persisted to the present day.

It is well known that William Harvey (1578–1657) dared to dissent from the established ideas of the early Greeks, backing his statements with careful observation. Certainly Harvey knew that the human uterus was not bipartite, but he proceeded to test the application of the ancient theory to lower mammals. In his work *On Generation*, published in 1651, he speaks of detailed studies made on deer during gestation. "Males and females are met with indifferently in the right and left horn of the uterus. I have, however, more frequently found females in the right, males in the left horn; and I have made the same observation in does that carried twins, as well as in sheep. It is certain, therefore, that the right or left side has no appropriate virtue in conferring sex." Harvey also said that physicians of the time believed that the male and female both ejected their fluids into the uterus, and that the sex of the progeny depended upon predominance of one of the fluids. Highmore's (1651) writings (cf. Cole, 1930) are representative of this group. According to his theory, the blood stream collected particles from every part of the body, and these were carried to the genital glands where they were concentrated into the germ. There were, then, two kinds of seed, male and female, sex being determined by a struggle for dominance at the time of conception. This also was an ancient idea, first postulated by Hippocrates [translation by Coxe, 1846]. He believed that although the male seed was the stronger, it would be suppressed by a greater quantity of female seed. Whichever ultimately predominated determined the sex. Aristotle said that the ignorance of the "ancients" caused them to fall into error on this point.

The invention of the microscope by Leeuwenhoek (1632–1723) resulted in wild flights of the imagination because of inaccurate observations due to crudeness of the earliest instruments. There arose a large group of preformationists who claimed to see a completely formed individual or homunculus within the egg or sperm. Highmore believed that the germ represented the substance of the body, but not its visible form. Buffon (1769) thought that organic particles from all parts of the body assembled in the germ cell in the same positions which they had within the body, and that sex was a matter of dominance of the male or female germ. Hartsoeker (cf. Cole, 1930) in 1694 assumed that each seminal animalcule contained a tiny foetus, and that each of these in turn held an infinite number of smaller animals of the same species, both male and female. Hence the sex of every individual was predetermined since the creation of the species. De Launay (1698) opposed this view, supposing that the germ of each parent held a small foetus of the same sex as the parent. Only one of these would survive to determine the sex of the child.

The preformation doctrine and sexual theories which accompanied it were diehards, gradually succumbing only more

than one hundred years later because of their incompatability with advancing knowledge of cell division and evolution. In the meantime the ancient theories of the Greek physicians and philosophers were undergoing a rejuvenation.

In 1786 Henke performed unilateral ovariotomies on pigs, dogs, and rabbits, claiming that subsequent conceptions yielded only males from the right ovary and only females from the left. This is a modification of Hippocrates' theory concerning the uterus. Later Seligson (1895) similarly operated on rabbits, arriving at the same conclusion as Henke. Dawson (1909) confirmed the hypothesis and elaborated upon it. Using as evidence clinical cases, he concluded that either testicle may result in both sexes of offspring, but that the right ovary produced males and the left females. The following year Doncaster and Marshall (1910) tested Dawson's work. Rats which had undergone unilateral ovariotomy were found to produce both males and females from the single remaining ovary, since autopsy later proved that there had been no regeneration of the removed tissue. King (1911a) further blasted the ancient theory. She made cross matings of hemi-spayed and hemi-castrated albino rats, all matings resulting in both sexes of offspring. There were 42 males and 41 females produced, a proportion very close to that of a normal rat population. Parker (1914) examined pairs of unborn pigs in the bifid uteri of the mothers. He found the sexes distributed as follows:

	♂♂	♀♀	♂♀
At division of horns of uterus..	252	240	456
Horn next to right ovary.....	228	209	434
Horn next to left ovary.....	216	208	447

Since it is extremely improbable that all of the males found in the left side had come from eggs of the right ovary, and that the females in the right side had migrated from the left ovary, Parker considered this data as disproof of Seligson's work. As was pointed out by Marshall (1925), birds normally have only one functional ovary, and yet produce offspring of both sexes.

There have been several cases among women where a conception subsequent to the removal of one ovary has resulted in births of male progeny from the left side, or of females from the right. Rawlings (1922) cited a case in which a male was produced by an egg from the left ovary. It is obvious that the original hypothesis was a false one, and early experiments alleged to support it are now disregarded.

Other theories of sex determination arose to take the place of the time-worn idea that each ovary produced only offspring of one sex. Most of these were variations of the thesis that sex depends upon the time during the ovarian cycle at which the conception takes place. The studies relating to this subject will be discussed in a later section concerning the sex ratio.

Nutritional theories have often been propounded to explain sex determination and differential sex ratios. In his studies on *Hydatina senta*, Nussbaum (1897) said that if a female is well nourished shortly after emergence, she will later produce large eggs which develop into females. On the other hand, a poorly fed female will produce small male eggs. Some biologists believed that not the quantity of nourishment, but the chemical constituents of the food determined sex. The writings of Schenk (1902) are representative of this group. According to his theory, a diet high in carbohydrates fed to the mother before conception results in the birth of female children, whereas

a protein diet is responsible for males. The investigations of King (1907, 1909) on *Bufo lentiginosis* did much to undermine the notion that sex can be determined by feeding. She found no significant difference between the proportions of sexes among normally fed tadpoles and among over-nourished tadpoles, nor did a protein or carbohydrate diet exert any influence in sex determination in the toad. Reed (1913) criticized nutritional experiments of Geddes and Thomson (1889) which were alleged to give positive results as being a contradiction to Weismann's ideas on non-inheritance of acquired characteristics. The basis of the criticism is that nutrition can at most result in temporary somatic modifications and hence cannot affect the offspring in any manner. This in itself does not disprove, however, that nutrition may have an influence on characteristics of the progeny subsequent to fertilization, especially in viviparous organisms. Nutrition is certainly not the dominant factor in sex determination, but its influence on the sex ratio still remains a moot question. In man the problem is not open to laboratory experimentation, so it has been approached by studying the proportions of sexes during wars and famines, and among different economic classes.

Superstitions have always had a large influence on the minds of people desiring a child of a certain sex. Økland (1932) discussed these at length. Two thousand years ago the idea started that phases of the moon determined sex, a figment that is not yet outworn. Another myth postulated that if conception was properly timed with the state of wind, rain, or temperature, one could have a boy or girl at will. Wishful thinking or temperament of the parents has had a prominent place in such superstitions. Passionate wives supposedly have male children, and likewise vivacious wives and phlegmatic husbands. Primitive peoples perform ceremonial dances, while at one time Europeans buried perforated coins outside the door in order to have sons. Another bit of folklore held that the husband who had intercourse with his boots on would infallibly be blessed with an heir to his name. With the increasing education of the masses such superstitions have diminished. Even more plausible theories have been abandoned or have undergone radical changes as a result of important findings at the turn of the last century.

THE CHROMOSOME MECHANISM IN SEX DETERMINATION

The discovery of unequal sex chromosomes by McClung (1902) while studying spermatogenesis in *Xiphidium fasciatum* and in the Acrididae generally, inaugurated a new era of thought concerning sex determination. The chromosome mechanism is adequately described in a number of books on genetics. For the present section general references have been made to Lindsey (1932), Sinnott and Dunn (1939), Waddington (1939), Marshall (1925), and Sturtevant and Beadle (1939). Within the body cells and primordial germ cells of an organism there are found like pairs of chromosomes. The exception to this is in the sex chromosomes or allosomes where the members of the pair are dissimilar in one of the sexes. In most animals the female has the chromosome complex XX while the male has the unequal XY or merely a single X. In Lepidoptera, birds, and some fishes the opposite situation exists, the female being heterogametic. During meiosis there is a division of the pairs of sex chromosomes and autosomes, each gamete receiving the haploid number. In those organisms where the male is the heterogametic sex, half of the gametes of the male contain the

X chromosome, and half the Y chromosome. In the female all gametes are of the X type. At fertilization there is a restoration of the full complement. According to the laws of probability, if the meeting of the ovum with either one of the two types of spermatozoa is purely a matter of chance, half of the zygotes formed should be of the XX or female type and half of the XY or male type of sex chromosome complex. Barring the intervention of subsidiary factors which might override this simple mechanism, or influence the embryo during gestation, one would expect in a sufficiently large population that the ratio of males to females at birth would be 1:1. However, there are such complicating elements, some of them proven, others theoretical. These may be divided into three classes: (1) factors disturbing the normal mechanism of gametogenesis, (2) factors preventing the purely chance meeting of the ovum with either type of sperm, and (3) factors acting upon the embryo or foetus during gestation.

The first of these follows from the observation (Bridges, 1932; Witschi, 1934; and Goldschmidt, 1934) that the allosomes are not alone as the genetic determiners of sex, but that there are also sex factors located on the automosomes. The quantitative relationship between allosomes and autosomes determines whether the individual will possess a preponderance of factors for maleness or for femaleness. Bridges showed this relationship in *Drosophila melanogaster*. By a chromosome division without a separation into two nuclei, a tetraploid condition arises. Subsequent meiosis results in diploid eggs. When such eggs are fertilized by X sperms, triploid females result. The offspring from such triploid females may represent a variety of chromosome complexes and a wide range of sexuality depending upon the ratio between the number of automosomes and the number of X-bearing chromosomes. The automosomes quite probably have an influence on sex determination in higher animals, and obtain varying degrees of expression through abnormal gametogenesis.

The second class of factors which may have a part in determining sex are only speculative. It will be shown presently that in all probability males and females are not conceived in equal numbers and several theories have been advanced to explain this apparent upset to chance fertilization. Sturtevant and Beadle (1939) suggested that although the two kinds of sperm are produced in equal numbers, the most likely reason for deviation from a 1:1 ratio of sexes at conception is a differential rate of motility between the X and Y types. Thus the eggs would be more frequently fertilized by the sperm with the greater rate of speed. Another possibility is differential selectivity and viability of the male-determining sperm. King (1918) selected rats for high and low sex ratios. She accounted for the difference in the two strains as being due to selective fertilization, the ova in the high ratio strain having a greater receptivity to male-producing sperm. These theories are discussed by Hartman (1939) who considers them unsusceptible of proof or disproof and likely to be perpetuated in the manner of folklore.

Studies on the sex ratio at birth have dealt almost exclusively with environmental factors which, acting upon the mother, produce a differential mortality between male and female embryos. These will be considered in detail in following sections. Whereas this is a process of selective elimination, a few other works indicate that determination or differentiation of sex may be influenced by factors acting subsequent to fertilization. Thus

Shull (1925), studying the rotifer *Hydatina senta*, the aphid *Macrosiphum solanifolii*, and the cladoceran *Simocephalus vetulus* in their bisexual and parthenogenetic cycles, concluded that sex determination may depend partly upon differences in the metabolic level, but also upon other physical or chemical factors of the environment. In *Bonellia viridis* the chromosome mechanism, if present at all, must be very weak, for Baltzer (1937) found that extra-genetic conditions play the predominating rôle in sex determination of the larvae. If they become attached to the body of the adult female they develop into males, but separation from the body results in female development. This may be due to a secretion by the female or to other environmental influences such as differential acidity of the water.

Goldschmidt (1916, 1934) found that all of his studies based solely on sex chromosomes fell short of an adequate solution to the problem. Working with the gypsy moth *Lymantria dispar* he formulated a theory that sex is determined by a quantitative relationship between male and female sex-producing factors, M and F. In the heterogametic female *Lymantria*, F is inherited through the cytoplasm; in other forms in the autosomes. M is found always in the X chromosomes. The F and M factors differ in strength among different races of the gypsy moth, and certain combinations result in intersexes. The sex factors are supposed to produce substances which control the tissues of the undifferentiated organism so that either male or female is produced according to whichever substance is in excess. In the intersexes, the turning point from male to female, or vice-versa, depends on the rate of reaction of the two substances which, it is suggested, may be enzymes.

In his extensive work on *Rana temporaria* Witschi (1929, 1934) concluded that, although sex in amphibians is determined by the quantitative relationship between genes of the sex chromosomes and autosomes, this mechanism is regularly eclipsed by environmental factors, resulting in sex races. The larval gonad possesses a cortex and medulla. During male development the cortex atrophies and the medulla becomes the inductor of sperm formation, while in female development the cortex becomes functional and induces formation of eggs. High temperatures prevent cortical growth, reversing the sex of female tadpoles by overriding the genetic factors. Low temperatures conversely inhibit medullary formation. Witschi (1934) did not believe that the morphogenetic substances belong strictly to the class of hormones in toads and frogs, but acted much like hormones in the newt. He based this conclusion on parabiotic twinning and gonad transplantations in *Triturus torosus*. King (1910) found that temperature may have an indirect action in the determination of sex in *Bufo*, but it was not the dominant factor.

Boring and Pearl (1918) described eight hermaphrodite fowl. These had embryonic or degenerating ovaries, and three of them were changing from a female to a male condition in behavior, secondary sex characteristics, and gonads. Such intersexes in birds have usually been attributed to sex hormones acting primarily during embryonic life, although the exact place of origin of such hormones has been contested. Moore (1925) believed this to be the case in the Leghorn chicken, and that a similar condition exists in mammals. Goldschmidt (1934) held that it is probable that vertebrates have embryonic hormones causing sex differentiation, although conclusive experimental data are lacking.

One of the most important studies on sex

hormones is that of Lillie (1916, 1917) on free-martin cattle. He brought forward embryological evidence that the heterosexual twins are dizygotic and the sterile free-martin is genetically a female. There is an anastomosis of the foetal circulations of the male and female embryos, so that the female organs of reproduction become degenerate due to the action of male sex hormones in the blood. The free-martin often develops some of the characteristics of the bull. Theoretically the reason that the reverse action never takes place is that formation of hormones in the male is more precocious than in the female.

Further evidence of the influence of hormones on sex differentiation may be implied by the well-known changes which occur in the body after castration or ovariotomy. Although such operations seem to result in the appearance of characteristics of the opposite sex, Marshall (1925) and Lipschütz (1924) declared that there is actually no evidence of a reversal, but rather an assumption of a neutral state. This neutral condition is more like the male than like the female among birds, so that avian castration results in only small changes, whereas ovariotomy appears to produce male characteristics.

Lipschütz formulated an hypothesis of sex determination based supposedly on the effect of hormones alone. According to this, the somatic constitution of mammals and birds is undifferentiated (bisexual or asexual) at the beginning of development. The condition is changed in a male or female direction during embryonic life by the differentiating sex gland and by this action only. This view follows from the observation that aninals that have undergone castration or ovariotomy can be femininized or masculinized by transplantation of gonads of the sex opposite from the original. Lipschütz alleged further support from his observations on intersexuality in man. He said "There is no need to assume special genetic factors for the male and female sex characters in mammals and birds. Transmission of the male sex characters by the female, and of female characters by the male can be explained on the assumption of a transmission of the characters of an asexual soma, which has the capacity to react in a male or female manner according to sex specific hormones, the latter being possibly not peculiar to the species." However, Lipschütz stated that the sex gland which produces such a hormone "is the morphological manifestation of the biochemical difference between a male and female fertilized egg." If one assumes that there is a biochemical difference, the question might be put as to what it is that originally determines whether a zygote is male or female. Examined under the light of genetic research, it seems certain that sex chromosomes are responsible for the determination of the zygote, and that the sex gland, which is in itself predetermined by the genes, gives rise to hormones that act as subsidiary factors of somatic differentiation.

Riddle (1927) pointed out that the metabolic requirements of the male are greater than those of the female, and suggested that this is a cause of the higher mortality of male embryos. The same view was expressed by Joyet-Lavergne (1929) in his earlier work on pigeons. Besides the possibility that the metabolic rate can effect the sex ratio at birth by a selective mortality, it has also been considered as an explanation of sex determination. Riddle (1932) advanced the theory that the X and Y chromosomes carry genes which influence oxidation. If the Y chromosome alone raises the oxidation rate in excess of a single X, then an XY complex will establish a higher rate than an XX combination. The XY gives rise to males and the

XX to females. There may also be genes in the autosomes which influence oxidation, but by and large they contribute equal effects, whereas the quantitative difference between X and Y results in unequal oxidation rates. The organism is capable of response to forced changes in oxidation at any time during its life. Consequently environmental or pathological conditions which disturb metabolism may account for sex-reversals. The case of a normal hen which became transformed into a functional rooster was explained by Crew (1923) as being due to a metabolic upset resulting from a tumor. Amar (1929) believed that carbohydrate feeding increased metabolism and caused a greater number of male births.

To summarize what is known about sex determination, it may be said that no single factor offers an adequate explanation, nor is the method constant throughout phylogeny. In some of the lowest forms the chromosome mechanism appears to be an unimportant factor and the question of sexuality is largely decided by the environment. Among insects, genetic elements obtain a high degree of control and sex results from the balance between genes of the autosomes and sex chromosomes. Intersexes arise from mutations or from chromosomal aberrations (Bridges, 1939). In the gypsy moth, however, the nuclear mechanism may be overridden by other sex factors, presumably enzymes or hormones. The situation becomes still more complicated among vertebrates. The sex chromosomes of amphibians can apparently be dominated by cortical or medullary secretions of the gonads, which in turn are subject to environmental influences. The degree of effect which environment may have in sex determination or differentiation among birds and mammals is largely unknown, but as Danforth (1939) says in his excellent review of the problem, "It is doubtful if there are many forms, even among the higher vertebrates that are entirely free at all stages from susceptibility to environmental influences on their sexual differentiation." It has been postulated that the metabolic rate may influence sex determination by means of genetic factors, and there is some evidence that environmental conditions which alter metabolism may in certain cases result in sex-reversal. However, true sex-reversal is rare among higher vertebrates, and it is doubtful that metabolism has a significant rôle in either determination or differentiation of sex. It has been conclusively established that the embryo is somatically indifferent during the early stages (Willier, 1939), and that subsequent sex differentiation results from the action of male or female hormones. The endocrine organs producing these hormones are determined by the chromosomal mechanism, and there is a close interdependence of the two factors. Although hormonal or environmental influences can in some cases dominate the nuclear factors during differentiation, it appears that determination of sex is primarily a genetic process taking place at the time of fertilization.

THE SEX RATIO

Why vital statistics "should be made known to the People, otherwise than to please them, as with a curiosity, I see not. Nor could I ever yet learn (from the many I have asked, and those not of the least Sagacity) to what purpose the distinction between *Males* and *Females* is inserted, or at all taken notice of?" John Graunt, who wrote this passage concerning the Bills of Mortality, nevertheless presented in 1676 the first observations known on the proportion of sexes in a large population. He found that from 1628 to 1662 there were christened in London 139,782 males and 130,866 females, the country

accounts also showing an excess of males. In London the proportion was 13 females for 14 males; in Hantshire (a country parish), 15 for 16; and in Cranbrook, 19 for 20. Graunt commented upon the greater proportion of male births in the city than in the country, and recommended the variation to the examination of the curious. He further observed that more males than females died of violent deaths, drownings at sea, war, and at the hands of justice, and more went unmarried to the colonies. Yet, because of the excess of male births, each woman might still have a husband. This he considered a blessing to mankind, being a natural bar to polygamy, for in polygamous unions a man must dominate his wives, financially and otherwise, to keep them at peace with each other and in awe of himself, "the poorest Subjects being the most easily governed." As to the male excess, "What the Causes hereof are, we shall not trouble our selves to conjecture, as in other Cases: only we shall desire, that Travellers would enquire, whether it be the same in other Countries."

It appears that Captain Graunt's own curiosity overwhelmed his initial pessimism. His recommendation has been taken seriously, and later students have found that the excess of males in human populations extends throughout the world. Moreover, in large groups and under general conditions, the proportions of the sexes are fairly constant. This is true also of other animals, although interspecific differences exist in the sex ratio, there being for some forms an excess of males, for others an excess of females. Such constancy within the species indicates a deep-seated mechanism of sex determination, such as the chromosome mechanism described in the previous section. However, it was also noticed that the genetic factors themselves may be modified, and that there are subsidiary factors which are capable, in some cases, of dominating the nuclear control of sex. One would thus expect intraspecific variations in the sex ratio at conception (or at the time of embryonic differentiation). This is referred to as the primary sex ratio. Variations in the proportions of sexes at birth, or the secondary sex ratio, are dependent not only upon the primary ratio, which results from the sex-determining mechanism, but also upon the sex-differential rate of prenatal mortality, a potential sex-selective mechanism. What are the specific conditions which may influence the sex-determining or sex-selective factors, and give rise to variations about the norm for the sex ratio in a particular species? Volumes of literature have been written in an attempt to answer this question.

Inheritance of the sex ratio

Whether or not sex ratios are inheritable is a topic of considerable controversy. Parkes (1926a) stated that the issue becomes a question of what factors resulting in a variation of the sex ratio could be of an inheritable nature. A large body of evidence has been accumulated to show that abnormally high or low ratios in *Drosophila* may be transmitted through successive generations. Rawls (1913) described flies which produced approximately two females to each male. The method of transmission in this case was shown by Morgan (1912) to be a sex-linked lethal factor on the X chromosome. The recessive lethal was always dominated in the female by the normal allele, but half of the sons received the fatal X and died before emergence. Sturtevant (Morgan, Bridges, and Sturtevant, 1925) described a mutation in *Drosophila affinis* which resulted in the production of very few males, and Gershenson (1928) reported a similar

case in *Drosophila obscura*. In the latter study 96 percent of the flies were females. Examination showed the causal factor to be a sex-linked gene which acted previous to fertilization. Apparently it caused almost total removal of the Y-type of spermatozoa, behaving much like a gametic lethal. The presence of an inheritable character producing an excess of males in the ratio 5.5:1 was shown by Redfield (1926). She believed that the lethal took effect during the egg stage, probably within the mother's body. Bridges (1917), Mohr (1923), Bonnier (1923), and Mohr and Sturtevant (1919) gave data on other lethals which caused transmissible variations in the sex ratios of *Drosophila*.

Woods (1906), writing shortly after the discovery of sex chromosomes, concluded that sex determination was not according to Mendelian principles, nor was the sex ratio inherited. Using genealogies of the reigning houses of Europe, he made a fourfold correlation table of parental and filial fraternities which showed either an excess of males or an excess of females. The correlation was positive, but statistically insignificant. A similar investigation was conducted by Heron (1906), although he correlated sex ratios of individual sibships in two successive generations. The four correlations made for man were, as in Woods's study, positive but statistically insignificant. Stud book entries were likewise subjected to this treatment with the same results. Heron stated that inasmuch as this material showed no sensible inheritance of sex, variations in the ratio are not racial, but must be associated with environmental factors. Pearson analyzed Weldon's (1906) data on Japanese waltzing and albino mice. He made correlations of filial sex ratios with parental and grandparental ratios, finding positive correlations in the first case, and negative in the second, all low and insignificant. This agreed essentially with the findings of Woods and Heron.

These three studies are subject to the same criticism. The multiplicity of factors which may influence sex determination or sex ratio was at that time unrecognized, and no account was taken of such factors. With unselected matings in a general population a high correlation of sex ratios between generations could hardly be expected. The authors failed to take cognizance of the fact that the sex ratio *per se* may not be inherited according to Mendelian laws, but that, as previously mentioned, there is a question of what inheritable factors may give rise to sex ratio variations. An investigation by Willoughby (1931) showed a probable tendency toward inheritance of the sex ratio. The correlation between the sex ratios in fraternities of the individuals considered and the sex ratios of paternal and maternal cousins were particularly high, being $.52 \pm .06$ and $.68 \pm .04$ respectively.

Moenkhaus (1911) selected *Drosophila ampelophila* for high and low sex ratio strains. He found a normal ratio of 1 male to 1.126 females in 26,933 flies. Starting with two pairs from nature he selected for six generations. The high male strain remained only slightly above the norm during each generation. The strain high in females continued high, however, being as great as 1 male to 2.17 females in a particular generation. Another generation threw 1 male to only 1.36 females, and Moenkhaus explained this as an unfortunate selection of parents. The conclusion was drawn that sex ratio is a quality that is "strongly transmissible and amenable to the process of selection," the female being almost entirely responsible. Warren (1918), in criticizing this experiment, alleged that a low ratio in a particular generation could not be due to a poor selec-

tion of parents, since selection has a cumulative effect. The criticism seems not well grounded when one considers how few generations the experiments covered. There may easily have been a pair of flies which would throw an unfavorable ratio. Warren believed that the maintenance of a high proportion of females was due to a sex-linked lethal in the female strain, although there is now no way by which this can be conclusively demonstrated. Warren's own selection experiment with the same species showed only slightly divergent ratios, by which he concluded that selection cannot alter the proportions of sexes.

A much more extensive selection experiment was conducted by King (1918) on albino rats. The mean ratio of out-bred rats was found to be 104.6 males per 100 females. Animals were inbred for six generations before selection began, and the sex ratio differed by an insignificant amount from the established norm and from a control group. This agreed with Moenkhaus's (1911) observation on inbreeding in *Drosophila*. Having established that inbreeding in itself had no correlation with masculinity, King started selection in the seventh generation from a single pair mating. Her results are summarized in Table 1.

Rats from each series were later bred to stock animals and showed the tendency of the particular series, but to a lesser degree. Moreover, the female seemed to exert the greater influence on determination of the ratio. King agreed with Moenkhaus that sex ratio is probably an inherent characteristic and amenable to selection.

Although the *Drosophila* studies were based on too few generations and insufficient numbers to warrant any conclusion as to the effect of selection, the albino rat experiment largely overcomes objections. Tentatively, at least, the interpretations seem justified.

Darwin (1874) considered this problem in *The Descent of Man and Selection in Relation to Sex*. The method of selection to which he had recourse was the practice of female infanticide among certain populations. The reasoning was that families having an excess of females destroyed a greater proportion of the children than families having an excess of males. If sex ratio is a transmissible character, then the bias in favor of a male-producing race should increase with successive generations. Darwin found on good authority that female infanticide which had been long practiced in New Zealand, was almost extinct by 1835 and entirely so by 1844. In a limited area where the population was carefully studied, the "non-adult" males numbered 178 and the "non-adult" females 142, a sex ratio of 125.3 to 100. These figures are for the year 1858. If "non-adult" meant children under 14 years of age, the ratio may indicate a selection, especially inasmuch as there is a higher male than female mortality during infancy and childhood. However, the numbers are meagre, and if

TABLE 1

Showing, by generation groups, the sex ratios in the inbred litters of male and female lines

(Adapted from King, 1918)

GENERATION	SERIES A (HIGH STRAIN)		SERIES B (LOW STRAIN)	
	No.	Sex ratio	No.	Sex ratio
1–7	930	110.4	932	109.0
8–10	779	112.5	795	92.5
11–13	953	130.7	876	84.0
14–16	999	126.5	888	76.9
17–19	1144	121.7	1083	77.3
20–22	1270	122.0	1102	84.6
23–25	1129	123.5	1149	78.7
8–25	6274	122.3	5893	81.8

the "non-adult" category included individuals over 14 years of age, the proportions are possibly distorted by the infanticide. Certainly the adult figures which Darwin gives for New Zealand are of no significance in the problem. In the Sandwich Islands infanticide had ceased by 1819. In 1839 the males under fourteen years in Kauari and under 18 in Oahu numbered 1797, while females of corresponding ages numbered 1429, giving a sex ratio of 125.75. A census of all the islands in 1850 gave the number of males and females under 17 years of age as 10,733 and 9,593 respectively; a ratio of 112.3 to 100. In 1872 the adult population had a ratio of 125.36 males to 100 females. Darwin was of the opinion that due to differential rates of mortality, the ratios would have been even higher had they referred to births. He concluded that there is reason to believe that selection of this sort tends to make a male-producing race, but did not believe that a similar process occurring in nature accounted for the deviation from a 1 to 1 ratio in a general population for either man or other animals.

Although the information is meagre concerning inheritance of the sex ratio, the weight of the evidence indicates that lethal factors and probably unknown others of an inheritable nature give rise to transmissible variations from the normal sex ratio of the particular species.

Hybridization

Several studies have indicated that hybridization increases the proportion of male offspring. King (1911b) mated albino rats with wild Norway rats and found a sex ratio of 119.07 among 425 births. The ratio used as normal for the albinos in the experiment was 106.46. King assumed that *Mus norvegicus* had approximately this same proportion of sexes, although a later study (King, 1924) demonstrated that the wild rat has a much lower ratio. A high proportion of males was also found in hybrids of the deer-mouse, *Peromyscus*, by Sumner, McDaniel, and Huestis (1922). In 2930 offspring of pure matings they found 93.27 males per 100 females, whereas the hybrids had a ratio of 104.76 per 100. The difference divided by its probable error gave a critical figure slightly less than 3. It was suggested that the high hybrid ratio might have been due to a selection of male-producing spermatozoa by the ova, or that greater vigor resulting from cross-breeding may have influenced the metabolic rate. Theoretically this would have a differential effect upon male and female foetuses. The view is of course purely speculative. Guyer (1909) also thought that metabolism might be the determining factor in producing the higher sex ratio of hybrids. Hypothetically, the incompatibilities between two dissimilar germ plasms would result in a lowering of the metabolism of the embryo. This, Guyer believed, would be hostile to female development. However, in view of the greater metabolic requirements of the male, it would seem at least equally plausible that decreased metabolism might be inimical to male development. The original proposition is probably false. Guyer's study of museum specimens included 51 hybrid birds of known sex. Of these, 33 were pheasant crosses, among which there were but 4 females. The remainder, Guinea fowl × chicken and pheasant × chicken, were all males. This very abnormal distribution of sexes may have resulted from a selection of male specimens for museum pieces. Moreover, as the author realized, there is no indication of the probable ratio at the time of hatching. Riddle (1925) reported no females in 61 offspring of common pigeon × ring dove crosses. A highly specialized case wherein selective elimina-

tion of foetuses affects the sex ratios of hybrid births, is that of the bison and cattle crosses described by Babcock and Clausen (1918). The pelvic dimensions of the domestic cow are unsuited to the shape of the male hybrid foetus. Consequently the males seldom reach full term, and live births are predominantly female.

Pearl and Pearl (1908) studied the ratios of cross matings as computed from vital statistics of Buenos Aires, 1896 to 1905. They found that the Italian × Argentine ratio of 105.72 ± .46 and the Spanish × Argentine ratio of 106.69 ± .74 were, except for one comparison, significantly higher than the proportions of sexes among pure matings of these groups. The single insignificant difference was in the same direction as the others, and as the authors stated, does not invalidate the general result. The Buenos Aires data are particularly valuable for such a study since the pure Argentines are, in a genetic sense, strictly homozygous. This study considered the possibility that the greater excess of males in the cross matings was due to a difference in the proportion of stillbirths, age of parents, or environmental conditions. The conclusion was reached that these factors probably played an unimportant part, if any, in determining the observed variations. Lewis and Lewis (1906) also concluded that cross matings in the Buenos Aires population produced high sex ratios. Studies by Little (1919, 1920) of hospital births in New York City supported the earlier investigations. This author found a ratio of 106.27 in 5,753 progeny of pure matings, while that of 1,303 hybrids was 121.56. The difference was 6.76 times its probable error. These figures included stillbirths and abortions, the percentages being 6.17 in pure matings and 3.98 in crosses. Little explained the lesser amount of reproductive wastage in cross matings as being characteristic of greater vigor among hybrids. He alleged that there was less chance for duplication of adverse recessives in zygotes resulting from different stocks.

A study by de Jastrzebski (1919) failed to support the view that hybridization increases the proportion of males. In this work the sex ratio of offspring from mixed parentage was midway between that of pure native and pure foreign for Uruguay, lower than pure matings for the United States in 1915, and higher than pure matings for New York City. The inconsistency of the results led de Jastrzebski to conjecture that the sex ratio of hybrids depends on the nature of the cross. It must, however, be recognized that the registration of vital statistics in Uruguay has not been, in the past, adequate for any critical investigation, much less when these statistics are broken down into nationality groups. When we couple with this fact the knowledge that a "pure" mating in the United States is practically non-existent, it is obvious that de Jastrzebski's conclusions are unfounded. An investigation by Russell (1936) failed to show any consistent elevation of the sex ratio due to cross-breeding of immigrants in the United States. Ciocco (1938a) also analyzed the masculinity of hybrid births in the United States from 1917 to 1934. He made sex ratio comparisons of foreign × native white with pure foreign and pure native white for five nationalities. Of twenty such comparisons the hybrid sex ratio was higher than that of either of the pure matings in 5 cases, intermediate between them in 9 cases, and lower in 6 cases. This investigation suggested that cross mating does not always result in an increased proportion of males. Ciocco concluded that the hybrid ratio possibly was a resultant of the relative levels of the sex ratios of the nationalities of the parents. But his

studies on this point lack any great critical significance because of the fact that his "pure native white" groups are anything but "pure" biologically in the genetic sense. Statistically all United States white populations are more heterozygous than even any European population and much more so than any truly "native" population, in the sense of a primitive indigenous population. This applies equally to the United States data utilized by Russell.

A substantial body of data indicates that hybridization increases the masculinity of births in man as well as in lower animals. Studies which contradict this are found, upon close examination, to be based upon figures unsatisfactory for this type of study. It is quite probable that the greater biological vigor which accompanies hybridization is in some way responsible for the results observed.

STILLBIRTHS AND ABORTIONS

Darwin recognized as a weakness in his study the fact that in dealing with a primitive people the sex ratio might be higher because of greater ease in parturition and consequently fewer stillbirths. It is almost universally accepted among biologists that stillbirths and also abortions are more frequent among males than among females. The literature refers almost exclusively to human material. However, King (1921) found the sex ratio of 415 stillborn albino rats to be 129.3 males per 100 females whereas the liveborn ratio of 31,670 individuals was 104.1. Among cattle, Johannson (1932) reported that 57.33 percent of all stillbirths and 58.86 percent of all abortions were males. Goehlert (1882) showed that in horses 135,826 living foals were in the ratio 96.57 males to 100 females, while stillbirths showed 106 to 107 males for each 100 of the opposite sex. For man, Bertillon (1875) reported 134.1 as the sex ratio of stillbirths in Belgium during the years 1851–1860. Auerbach (1912) placed the figure for Budapest as 123.6. Wolda (1927) gave 117 to 133 as the range of sex ratios among stillbirths in Holland during the years 1901 to 1910. The average for Canada, 1925 to 1930, was given by Wyllie (1933) as 132 males per 100 females. Heape (1908a) put the stillborn sex ratio of Cuba at 144.45. For the United States, Ciocco (1938a, 1938b) gave the mean proportion of 68,932 stillbirths during the ninth month of uterogestation as 1346.9 males per 1000 females in the years 1925 to 1934. The sex ratios of abortions according to the period of uterogestation are given in Table 2.

TABLE 2

The sex ratio of abortions according to the duration of pregnancy

MONTH	SEX RATIO OF ABORTIONS				
	Ciocco (1938b)	Greulich (1931)	Schultz (1911)	Auerbach (1912)	Bertillon (1893)
1	228.00	357.48	—	—	180.0
2	431.13		—	452	
3	361.00		121.0	322	
4	201.22	223.14	117.5	229	
5	139.61	139.36	109.6	163	118.5
6	122.74	128.91	87.5	116	112.1
7	112.40	116.60	108.5	116	116.4
8	124.75	125.26	133.3	—	108.7
9	134.69	137.43	167.6	—	131.7
10 and over	133.28	150.13		—	—

The sex of embryos aborted during the early months of gestation is difficult to determine from a superficial examination. In Ciocco's material the sex of abortions during the third month was recognized in 63.2 percent of the cases, during the second month in 27.8 percent, and during the first month in only 10.1 percent. These data and those of Greulich, Auerbach, and Bertillon were obtained from official statis-

tics. The figures given by Schultz, although based upon fewer cases (1,410), resulted from careful examination of each foetus. Consequently the ratios which he gave during the third and fourth months may more closely approximate the true proportions than do the others. Auerbach's figures for these months were estimations and are probably excessive.

Material such as the above has been taken along with citations as to the total incidence of abortions at various periods of uterogestation, and from these figures estimations have been made of the sex ratio at the time of conception. Assuming a live born proportion of 105.5 males to 100 females, Schultz (1921) computed the primary sex ratio as 108.74. This differed but little from his earlier (1918) estimation of 108.47. Auerbach placed the figure at 116.4, while the ratio based on Ciocco's data is 137.22. Such wide variations are to be expected in view of the unreliability of the material upon which these ratios are founded. Nevertheless, they are an indication of the amount of deviation from the simple 1 to 1 ratio that would be expected from chance fertilization of ova by either X- or Y-bearing spermatozoa, if these exist in equal numbers. Boldrini (1936) dissented from the idea that the sexes are conceived in unequal numbers. It was his conviction that many abortuses at all stages, and particularly during the early months of gestation, are described as males when they are actually females. The excess of males at birth, according to Boldrini, is due to a higher rate of prenatal mortality among females. The data from which he drew his conclusions are far from convincing, and the results are incongruent with all other observations.

Considerable controversy has existed as to the reason for the higher incidence of mortality among male embryos and foetuses. Holmes (1926) believed that the male is inherently weaker than the female, constitutional differences between the sexes being the discriminating factor. Wyllie (1933) and Auerbach (1912) also considered the male to possess an inferior vitality. King (1921) advanced the hypothesis that the difference in constitutional vigor between male and female has its foundation in a difference in the chromatin structure of the two types of zygote. It has been postulated that sex-linked recessive genes are responsible, and Holmes believed this to be a real though minor factor. An anonymous (1937) writer was also a proponent of the theory of sex-linked semi-lethals among those organisms where the female is the homogametic sex. The lower viability of males in species where the female is heterogametic was accounted for by the greater metabolic requirements of the male organism. Riddle (1927) revolted at the thought that the male is inherently the weaker sex. The mere fact that more males die during gestation does not prove that they are genetically inferior unless it is shown that male and female embryos are subject to identical conditions in utero. Since the male has greater metabolic and nutritional needs, this can, according to Riddle, serve as a prenatal death selector for sex. Furthermore, filterable female sex hormones of the mother probably have an adverse effect upon the male embryo. Most other writers who have disagreed with the view that the male is inherently weaker, have believed that the difference in sex incidence of prenatal mortality is due to the larger size of the male foetus. Greulich (1931) attributed the rise in the sex ratio of abortions after the seventh month to this cause. Data from the Clinique Baudelocque presented by Pinard and Magnan (1913a, 1913b) showed the numbers of deaths during gestation to be 618 males and 611

females; during labor, 467 males and 351 females; and within 11 days after birth, 867 males and 614 females. These writers concluded in opposition to other studies that there is no difference in sex mortality during pregnancy. They attributed the difference during and shortly after labor to an excess of obstetrical traumatism among males as a result of their greater weight. It is quite conceivable that the larger size and greater metabolic requirements of the male may be selective for death in the last few months of pregnancy, but it is difficult to believe that the maternal organism is incapable of responding to the slight difference early in gestation.

Ciocco (1938b) classified prenatal mortality and sex ratio according to the causes of death, and in relation to the duration of uterogestation. It is to be expected that the size of the foetus would have the greatest effect upon mortality caused by difficult labor. Ciocco's figures for this category are as follows:

GESTATION PERIOD	TOTAL	8 MOS. AND OVER	5–7 MOS.	4 MOS. AND UNDER
Percent of males.....	62.7	62.9	55.9	70.0
Percent of prenatal mortality.....	100.0	96.9	3.0	0.1

Little significance can be attached to the sex ratio for four months and under as it must be based on very few cases (probably 16 since the total number of stillbirths due to difficult labor amounted to 15,755). Furthermore, the sex and cause of death of such young embryos are difficult to determine with reliability. Of all the causes of death, the sex ratio 62.9 is the highest for the group 8 months and over, while 55.9 is one of the lowest in its period. Thus it would appear that foetal size affects the sex ratio of stillbirths predominantly during the final months of pregnancy. In regard to the total sex incidence of abortion, Ciocco found the masculinity to be lowest in cases of defective embryonic development (malformations) and high as a result of faulty or diseased placental structures. The highest ratios were associated with malpresentation and difficult labor. Ciocco stated:

> One of the most important correlaries to be drawn from these observations is that apparently there is no evidence to justify the current assumption that the males under all conditions are more liable to stillbirth. Consequently the theories which seek to explain the so-called masculine inferiority on the basis of chromosome structure, metabolism, vitamin requirements, etc., need to be modified considerably in order to be consistent with the above findings.

It is probable that all of the factors considered have some part in the differential elimination of foetuses of the two sexes, but the difference cannot be attributed exclusively to any one of them. The data are not only inconclusive concerning the later months of pregnancy, but the greatest handicap lies in the void of knowledge existing in the early periods when, apparently, the masculinity of abortions is at a maximum.

By far the most widely accepted explanation for variations in the sex ratio at birth is that of differential rates of prenatal mortality. Since the sex incidence of abortions and stillbirths is greater among males, a group which has a low intrauterine death rate will demonstrate a higher sex ratio among living offspring than a group which has a high proportion of prenatal deaths. It will be noted that such an assertion, if put forward as the sole cause for variations in the ratios of sexes at birth, requires proof that the primary sex ratios and also the sex incidences of antenatal mortality of the groups under consideration are equal, or differ in an amount or direction which can have no significant part in determination of the observed variations. Because such a proof

is very nearly impossible, it is not surprising that it has been overlooked or taken as a postulate, largely because variations in prenatal mortality rates are such a ready explanation of differences in secondary sex ratios. If reproductive wastage is the sole factor, any increase in the rate of stillbirth and abortion should be accompanied by a decrease in the proportion of males among live births. A small amount of data by Parkes (1924a) on mice suggested that this might be true. MacDowell and Lord (1925) made a similar study of mice on a much larger scale, and found no relationship. Ciocco (1938a) investigated the problem in man by calculating the co-variation between changes in the prenatal mortality rate and the live birth sex ratio for successive years in selected areas of the United States. He found that in 50.8 percent of 236 possible changes, both the incidence of prenatal mortality and the secondary sex ratio increased or decreased together, while in 49.2 percent they changed in opposite directions. There was no statistically significant difference from the expected 50 ± 3.3 percent. Similar insignificant differences were obtained in co-variations between the sex ratios of live births and of stillbirths and abortions. Although the amount of prenatal mortality in some cases has an important part in determining variations of the secondary sex ratio, it certainly cannot be the only cause. In view of the rigorous requirements of a proof that it is the sole cause, dogmatism on this point would seem a poor policy. Nevertheless, a number of writers have alleged that such is the reason for differences which they observed in secondary ratios, without giving consideration to other possible factors.

VARIATION IN THE SEX RATIO AT BIRTH

Supposedly normal sex ratios of various animals are presented in Table 3, along with reference to the source. These ratios are confined to dioecious and non-parthenogenic forms and of necessity largely concern laboratory and domesticated animals. Information is so meager on sex ratios of forms lower than birds (except for a few insects and amphibians) that it is doubtful if these data can be considered as representative even for the species, to say nothing of the general phylogenetic level. The most widely studied of the insects, *Drosophila*, under normal conditions has a surplus of females at emergence. The sex of amphibians is usually determined in only a fraction of the total offspring, and the ratio varies according to the period of life at which they are sexed and with environmental conditions (Witschi, 1934; King, 1909). Sex reversal and hermaphroditism are common in amphibians, and there is wide geographical variation in the proportions of the sexes. The problems involved cannot be discussed in the present paper, and a mere listing of the sex ratios would be meaningless. The sex ratio of the domestic fowl has been studied with sufficient care to warrant the conclusion that the proportion of females is always greater than that of the males. Heape's (1907) figures on the canary were obtained from two breeders. It is doubtful if these are of any great value inasmuch as the numbers are not only small, but the parent birds were kept under widely diverse conditions. The difficulties of obtaining correct sex ratios for wild birds were overcome by McIlhenny (1940) in his study of the Gulf Coast Redwing and Boat-tailed grackle. These birds are both of the family Icteridae, both lay three eggs to the brood, and the sex can in each case be determined in the nestlings. In those nests which contained a full complement of offspring the redwing had a male percentage of 76.9 while that of the grackle was 30.3. The male percentage of the trapped and banded birds of these same

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TABLE 3

Sex ratios among lower animals

DESCRIPTION	NUMBER	♂♂ PER 100 ♀♀	AUTHORITY
Canary	68	353.3	Heape (1907)
Gulf Coast Redwing	420	332.9*	McIlhenny (1940)
Dog, German Shepherd	1,400	124.3	Whitney (1939)
Lepidoptera, *Bombyx* (several species)	1,695	122.7	Darwin (1874)
Pig	2,357	111.8	Wilckens (1886)
Mouse, albino	2,903	111.0	Parkes (1926b)
Dog, greyhound	6,878	110.1	Darwin (1874)
Rat, albino	1,001	108.0	Slonaker and Card (1923)
Cattle	4,900	107.3	Wilckens (1886)
Guinea pig	7,989	106.92	Ibsen (1922)
Cattle	124,166	106.3*	Johansson (1923)
Rat, piebald	2,845	106.0	King (1924)
Pigeon	1,648	105.23	Cole and Kirkpatrick (1915)
Rat, albino	2,818	104.6	King (1918)
Rat, albino	31,670	104.1	King (1921)
Mouse, albino	2,322	103.1*	MacDowell and Lord (1926)
Coleoptera, *Tribolium confusum*	2,500	102.8*	Schneider (1940)
Pig	5,380	102.6	Parker (1914)
Mouse, Japanese waltzing and albino	479	102.0*	Weldon (1906)
Cattle	1,313	100.2	Pearl (1917a)
Fish, *Gambusia*	2,593	100.2	Hildebrand (1927)
Horse	25,560	99.7	Darwin (1874)
Sheep, Cheviot and black-faced	50,685	97.9	Darwin (1874)
Guinea pig	2,014	97.8*	Schott and Lambert (1930)
Diptera, *Drosophila melanogaster*	31,097	97.8	Lawrence (1940)
Diptera, *Drosophila ampelophila* (*melanogaster*)	15,002	97.7	Hyde (1914)
Sheep	6,751	97.4	Wilckens (1886)
Mouse, *Peromyscus*	5,050	97.37	Sumner, McDaniel, and Huestis (1922)
Horse	16,091	97.3	Wilckens (1886)
Sheep, Leicester (one year old)	8,965	96.7	Darwin (1874)
Horse	135,826	96.57	Goehlert (1882)
Pig, Duroc-Jersey	16,233	95.4	Parkes (1923)
Mouse, albino	3,308	95.2*	Crew (1925)
Diptera, *Drosophila ampelophila* (*melanogaster*)	35,000	95.0	Warren (1918)
Domestic fowl	1,001	94.7	Darwin (1874)
Cattle	982	94.4	Darwin (1874)
Domestic fowl	20,037	94.4	Pearl (1917b)
Domestic fowl	2,396	93.8*	Jull (1924)
Domestic fowl	2,501	88.0*	Lambert and Curtis (1929)
Rat, wild Norway	1,862	85.8	King (1924)
Fish, *Coregonus albus* (adults)	841	84.8	Pearl (1916)
Mouse, albino	1,469	79.8*	Bluhm (1924)
Canary	200	76.99	Heape (1907)
Boat-tailed grackle	412	43.5*	McIlhenny (1940)
Siphonaptera, several genera (adults)	—	40–60	Shaftsbury (1934)
Terrapin (adults)	1,433	16.2	Hildebrand (1933)

* Computed from male percentage.

species was found to be a few points higher. Mayr (1939) in his study on the sex ratio of wild birds, considered such species variations to be a result of accessory genetic factors which modify the sex-determining mechanisms.

The several investigations on mice show a considerable amount of variation even in the albino strain. This differs from the situation found for rats in the excellent studies of King. The wild Norway was the only strain which had an excess of females. The dog has, in all studies, demonstrated the highest male sex ratio of any of the mammals. Among the ungulates the masculinities reported for the horse are low and quite constant, falling between 96 and 100. Gini (1908), in his extensive table on this animal, cited only one example wherein the number of males exceeded the number of females. Sheep likewise produce an excess of females, whereas cattle and pigs may vary in either direction from an equality of the sexes.

Data on sex ratios found in man are presented in Table 4.

These figures supposedly represent the condition found in a general population of each country under consideration. The table takes no account of secular variations. In all cases there is a higher proportion of male births than of female births. The ratio is seldom less than 104 or greater than 107 and is generally considered to average about 105.5 males per 100 females or 51.34 percent. Oppenheim (1926) reported a masculinity of 107.8 in 6698 Chinese births. Russell (1936) found the lowest sex ratios in Salvador and Japan, and the highest in Greece and Korea. The most comprehensive list of sex ratios for various countries was compiled by Gini (1908). Many of his figures were of early date and probably inaccurate due to faulty registration. Only a few of the ratios reported by him are given in Table 4, and these are taken from the period 1896 to 1900. The highest masculinities which he reported were for Greece, Rumania, and Spain, which are approximately 111. No very low ratios were reported, the remainder falling generally between 105 and 106.5. Gini stated that France demonstrated a low ratio in the years just preceding his publication, and this is born out by figures of Lewis and Lewis (1906) and of de Jastrzebski (1919). The former authors likewise found high

TABLE 4

Sex ratios in man

DESCRIPTION	MALES PER 100 FEMALES	AUTHORITY
Greece	113.2	Russell (1936)
Korea	113.1	Russell (1936)
Spain	111.1	Gini (1908)
Austrian Jews	109.1	de Jastrzebski (1919)
Cuba (whites)	108.4	Heape (1908a)
Bulgaria	108.3	Gini (1908)
Austria	106.5	Gini (1908)
Italy	106.5	Gini (1908)
Norway	106.4	Gini (1908)
Sweden	106.2	Gini (1908)
France	106.2	Darwin (1874)
United States (native whites)	106.2	Ciocco (1938a)
Finland	106.0	Gini (1908)
Germany	106.0	Gini (1908)
Holland	106.0	Gini (1908)
Denmark	105.7	Gini (1908)
France	105.5	Gini (1908)
Italy	105.5	Savorgnan (1933)
Cape Colony (whites)	105.4	de Jastrzebski (1919)
Japan	104.6	de Jastrzebski (1919)
England	104.5	Darwin (1874)
England and Wales	104.3	Russell (1936)
Japan	104.3	Russell (1936)
United States (colored)	102.8	Ciocco (1938a)
Cape Colony (blacks)	102.6	de Jastrzebski (1919)
Cuba (colored)	101.1	Heape (1908a)

ratios for Greece, Rumania, and Spain. The custom of presenting sex ratios by nationalities was criticized by de Jastrzebski, since these groups are not composed of racially homogeneous populations. Accordingly he divided Austria into several provinces, the people of which he claimed were relatively pure racially, although living under comparable conditions. These groups showed a variation in masculinity from 103.7 for the Serbo-Croats to 106.5 for the Italians. A similar division of India resulted in marked sex ratio differences for the several races.

All investigations on a sufficiently large number of cases show a lower masculinity of black races than of white. Gini pointed this out for Africa and for Queensland, while Heape's figures showed a wide variation between blacks and whites in Cuba. The low sex ratio of Negroes in these same countries and in parts of the United States was also demonstrated by de Jastrzebski. Although nearly every writer considered the differences to be a manifestation of racial characteristics, Huntington (1938) believed that they probably result indirectly from environmental factors. However, the weight of the evidence indicates that there are variations with race or nationality in man just as there are variations with species in lower animals.

SOCIO-ECONOMIC VARIATIONS

Students of sex ratio problems have generally alleged that socio-economic variations are due to differences in the amount of prenatal mortality. This view was taken by Winston (1932) who analyzed data on 5,466 families from *The Abridged Compendium of American Genealogy*. In a socially select group of 15,763 children, he found a sex ratio of 112.0. This high ratio was attributed to greater care and training of mothers with a consequent reduction of stillbirths. From United States governmental data the writer cited a stillborn sex ratio of 150.7 for 341 cases where the foetal death resulted from adverse economic conditions, namely, overwork of the mother. This stillborn ratio, it will be noted, is much higher than any of those previously mentioned. Crew (1937) stated that the sex ratio "is affected by social status, being higher in the upper and middle classes and lower amongst unskilled workers." He minimized the possibility that this resulted from any other cause than differences in prenatal mortality, and postulated that changes in the sex ratio at birth could be used as an index of the efficacy of public health measures such as slum clearance and social service. Punnett (1904), who studied the sex ratio in poor and wealthy classes in London concluded that a low masculinity was associated with low social status. Russell (1936) upheld this conslusion in his analysis of social classes in England for the year 1921.

The results of Lewis and Lewis (1906) conflict with the above studies. Among 2,877 Scotch professional families these authors found a sex ratio of 1032 males per 1000 females. In the working class the ratio was 1037 for 49,427 births. Offspring of commercial, agricultural and seafaring classes had sex ratios between 1066 and 1070. It must not be overlooked that the Lewises were severely criticized at the time of the appearance of their memoirs because all the Scottish data upon which their conclusions mainly rested were for live births only and took no account of stillbirths or earlier intra-uterine mortality. Kisch (1887) and Wall (1887) examined masculinity rates among European royalty, and found ratios of 107.7 and 107.4 respectively. Caution must always be exercised in interpreting sex ratios from royal genealogies, since there

may have been a tendency to underemphasize female births. Wall stated that masculinities were high among the peers in England, and also among the lowest classes whereas the middle classes contributed more to the female births. He alleged that these variations were due to differences in the age of the parents, a viewpoint shared by Russell. This subject of age of parents will be discussed in a later section.

The question of variation in sex ratio according to urbanity is a corollary to that of social and economic differences. John Graunt called attention to a greater proportion of male christenings in London than in the country. The urban ratio based on his data is 107.19 while that of a rural district is 105.61. The accuracy of the records at that time (1569–1664) is of course questionable. Düsing (1891), who investigated this problem in Prussia, found the reverse situation. His figures showed an almost linear increase in masculinity with decreasing urbanization. Lewis and Lewis (1906) demonstrated the same trend for births in Scotland. Omitting illegitimate births, the sex ratio was for principal towns 104.9, large towns 105.2, small towns 105.4, mainland rural districts 105.5, and insular rural districts 107.0. These authors did not consider the possible influence of prenatal mortality, but concluded that the factor of legitimacy had no part in determining the observed differences. Urban and rural sex ratios for a number of other countries were also given by Lewis and Lewis and this list was extended by de Jastrzebski (1919). Each of the ten countries studied showed a higher ratio in the rural populations than in the urban.

Vigor and Yule (1906) found no such clear-cut distinction in masculinity according to the degree of urbanization in England and Wales. Their data showed a high proportion of males in semi-urban populations and a lower proportion in large cities and in purely rural groups. Ciocco (1938a) divided his material for the United States into three similar categories. His figures were 1055.9, 1054.4, and 1053.8 males per 1000 females born in cities, incorporated places (2500–10,000 population), and rural districts respectively. Although this trend was the reverse of that found in most other studies, the differences were statistically insignificant. Russell (1936) adversely criticized the study of Vigor and Yule, and presented English statistics to show that decreasing urbanity is associated with increasing masculinity. This author supported his conclusion with United States statistics for the years 1927–1929, obtaining an urban sex ratio of 1057 $\pm$ 1.20 and a rural ratio of 1064 $\pm$ 1.15. Ciocco, in turn, believed that Russell's second figure resulted from a computing error, since his own computations based on the same material yielded sex ratios of 1057.2 $\pm$ 1.2 and 1057.6 $\pm$ 1.1 for urban and rural districts.

The bulk of the writings indicate that in a general population a high social and economic status is accompanied by a greater proportion of male births than is a low status. Also rural areas demonstrate a higher masculinity than urban districts, at least with respect to European countries. It is quite probable that part of the variation has its basis in differential rates of prenatal mortality, as has frequently been maintained. Bertillon (1875), Alberti (1934), and Taussig (1936) demonstrated that the incidence of reproductive wastage is higher for urban than for rural populations, and thus lend support to this explanation. These studies pertained to European data. Ciocco reported that this same condition exists in the United States, but alleged

that since he found no sex ratio differences according to urbanity in this country, the conclusion that such differences are primarily a function of prenatal mortality is subject to question.

ILLEGITIMACY

Lewis and Lewis (1906) found that in France, Belgium, and the British Isles the sex ratio of births from legitimate unions exceeded the ratio from illegitimate unions. A study by Heape (1908a) of the proportion of sexes produced in Cuba showed the same relationship. The legitimate and illegitimate matings resulted in ratios of 107.6 and 102.32 respectively in a total of 281,232 white births. The colored sex ratios were lower, but an even greater difference existed between legitimate and illegitimate births. Sadler (1830), Darwin (1871), Düsing (1884), and Bertillon (1896) had earlier commented upon this variation.

An immense amount of data dealing with this problem was amassed by de Jastrzebski (1919). The ratios covered roughly the period 1906–1915 for twenty-three countries. In the main the masculinity of legitimate was higher than that of illegitimate births, the exceptions being Norway, Sweden, Scotland, Spain, and Uruguay. England and Wales, Australia, and Denmark had approximately equal ratios in the two groups. Russell's (1936) figures for England and Wales, 1911 to 1930, showed only slightly higher sex ratios among legitimates, the differences being statistically insignificant. Ciocco reported similar insignificant differences in the United States. He maintained that figures tending to show a low sex ratio for illegitimate births in this country are the results of the racial composition of population, since about 50 percent of the illegitimate births are colored. Srdinko (1908), who found that the masculinity of legitimate births in Austria was lower than that of illegitimates, explained this as an effect of the composition of the population. Jews in Austria contributed a small share to the total reproduction, but, according to Srdinko, a large share to the illegitimate births. As the masculinity of Jews is reported to be high (Lagneau (1882), Lewis and Lewis (1906), de Jastrzebski (1919)), the illegitimates in that country would necessarily have a high ratio. The validity of such an assertion is perhaps questionable since illegitimate births among Jews have been shown by Lagneau and by Pearl (1939) to be relatively rare as concern Prussia and Russia in the former case and the United States in the latter. The most logical explanation of the variation in sex ratio between legitimate and illegitimate births is a difference in the incidence of reproductive wastage due to both natural and artificial terminations of pregnancy. The difficulty of obtaining accurate data on either abortion or illegitimacy is considerable, and trustworthy figures combining these are well-nigh an impossibility. However, a high proportion of conceptions resulting from illicit unions undoubtedly terminate in abortions. Bertillon (1893) gave data for Paris and Saint Étienne which showed higher rates of abortion among illegitimates than among legitimates for each month of pregnancy except the ninth.

It might be thought, from a superficial consideration, that there could be no selection for sex in criminal or therapeutic abortions, but actually in view of the difficulties of inducing terminations of pregnancy (Pearl, 1939) such a selection is not an impossibility. If the constitutional make-up of the male embryo is such that it is more susceptible to natural abortion, there is no reason to believe that it is not also more susceptible to induced

abortions, commonly resulting from illicit unions.

Order of birth

The variations in sex ratio associated with social conditions are sometimes ascribed to differences related to order of birth or age of parents. There is almost complete agreement that the sex ratio decreases with increasing parity. Lewis and Lewis (1906) found that masculinity of first births exceeded that of subsequent births in Scotland. When the material was classified according to birth order there were irregularities which the authors ascribed to the meagerness of the numbers, especially for later pregnancies. The figures for Budapest, reported by de Jastrzebski (1919), are 1051 males per 1000 females among first births and 1049 for later births. Savorgnan (1933) found the ratios of first and successive births to be 1059 and 1054 for Italy, 1074 and 1068 for Prussia, and 1069 and 1062 for Holland. Punnett (1904) presented data showing a regular decrease in sex ratio with advancing parity. This was supported by Siegel (1917), Parkes (1924b), and Ciocco (1938a). The most common belief is again that this is due to a difference in reproductive wastage. Supposedly the reproductive mechanism of mothers who have had a number of pregnancies is less fit than that of mothers who have had few conceptions. Alberti (1934) alleged that in Milan the incidence of abortion decreased with increasing order of begetting. He stated that this resulted probably because later births were carried on only by those women who were physically most fit and free from pathological conditions. However, Pearl (1933) showed an increase in the rate of abortion with increasing number of pregnancies of a selected group of highly fertile women. Hence there may be two factors concerned in abortivity in relation to order of gestation; first, a selection of women which tends to reduce abortions of later gestations and second, an increase in prenatal mortality, as a consequence of previous pregnancies. The findings of Alberti were detrimental to the theory that masculinity according to parity is a function of prenatal mortality, but no definite conclusion can be drawn from this single unsupported investigation. Huntington (1938) discussed the problem at length and maintained that the rate of abortion is a factor of prime importance. He based his conclusion on Kopp's (1933) confirmation of Pearl's observation that the incidence of spontaneous abortion increases with rising order of pregnancy. Huntington also believed that birth rank might have some influence on the germ cells and thus affect the sex ratio at conception.

There is a paucity of data on the sex ratios of birth orders among lower animals. Sumner, McDaniel, and Huestis (1922) found no differences in sex ratio according to litter order of *Peromyscus*. King (1924) reported that in rats the sex ratio gradually increased from 103.6 in the first and second litter groups to 141.6 in the ninth and tenth litter groups. After this there occurred a sharp decline. Although no statistically significant differences were found between successive litters, King believed that this lack was outweighed by the similarity in trend shown by four groups of rats, albinos, piebalds, extracted albinos, and extracted Norways. Obviously these studies are not consonant with the human material. Johansson (1932) reported a high sex ratio in first-born cattle, lower in second and third births, and an increase with later births. His figures indicated that the variations were due to differences in reproductive wastage.

AGE OF PARENTS

Sadler (1830) considered the sex ratio to be regulated by Divine purpose in such a manner as to insure maintenance of the population. According to his view, if, in a certain group, men married women much younger than themselves, many women would be left celibate and therefore unreproductive. In such cases a law of nature would increase the proportion of males at birth so as to compensate for the greater amount of male mortality due to the difference in the age at marriage of the two sexes. *A posteriori* considerations first led Sadler to assume that his theory was valid. He postulated that in urban marriages and in illegitimate unions the males and females were nearer the same age than in country and legitimate matings. Since his data showed low sex ratios of city offspring and of illegitimate offspring, the allegation was made that matings between parents of the most equal ages led to the lowest sex ratios.

Attacking the problem from another angle, Sadler presented statistics from peerage registers which showed an increase in the proportion of male births corresponding to an increase in the excess of the age of the father over that of the mother. Since there was a total of only 381 marriages, and these divided into seven age groups, it is obvious that the basis of the proof was gossamer. The same theory was upheld by observations of Hofacker (1827), and it existed for over half a century. Kisch (1887) suggested a modification of this Hofacker-Sadler law to conform with his own observations from data on European royalty. He believed that there was a marked increase in male births only if the husband was at least ten years older than the wife, and she at the height of her reproductive activity (20–25 years). Further figures on royal births (Wall, 1887) failed to conform with Sadler's observations, inasmuch as when the father was the older parent the sex ratio was 96.5 and when younger 119.2. Wall alleged that when males married later in life the masculinity of the offspring was low, and he attributed differences among social classes to this cause. It is difficult to see, from the irregularity of his figures, how he could have arrived at this conclusion. Other and more extensive investigations have resulted in complete abandonment of the so-called law. An early study by Stieda (1875) first cast suspicion as to its validity. Lewis and Lewis in their investigation discredited any association between masculinity and the relative ages of parents. Analysis of 1,436,762 Austrian births by de Jastrzebski revealed a sex ratio of 105.5 when the father was the older, and 105.7 when the mother was the older. Among more recent studies, Ciocco (1938a) reported insignificant variations in sex ratio according to relative ages of parents, while an examination of Russell's (1936) table likewise shows no association.

The general relationship which exists between age of parents and birth order of the progeny would suggest that there might be a decrease in sex ratio associated with an increase in the absolute ages of the parents. Sadler's material failed to show such a relationship and those of Wall and of Kisch were equally irregular. Because of the small number of births in each of these studies, not much weight can be attached to the results. Lewis and Lewis examined the sex ratio of several European countries and found no regularity of increase or decrease with advancing age of parents, either separately or combined. Other evidence has shown that whatever relationship may exist is in the direction of a decline in masculinity with increasing age. Lagneau stated that this was the

case, although he gave no figures to support his belief. Specht (1916) found a high sex ratio for children of young mothers. Hospital maternity cases were divided by Parkes (1924b) into five-year age groups. The 8384 births demonstrated, by and large, a decrease in sex ratio from 119.8 in the 18–22 year age group to 84.6 in the 43+ group. The youngest age, 13–17, with a ratio of 163.8, contained too few cases to be considered representative of that class. Ciocco (1938a) found no consistent trend in the sex ratio according to a quinquennial age distribution of parents. However, when a dichotomous division at 30 years of age was made, the ratios of Table 5 resulted.

TABLE 5

Sex ratio of births and age of fathers and mothers, United States B.R.A.

(From Ciocco, 1938a)

PARENT	AGE	1917–19	1920–24	1925–29	1930–34
Fathers	Under 30	1059.2 ±1.6	1061.2 ±1.1	1061.3 ±1.0	1058.1 ±1.0
	Over 30	1057.4 ±1.4	1054.4 ± .9	1056.1 ± .9	1053.3 ± .9
Mothers	Under 30	1058.5 ±1.3	1059.8 ± .9	1060.7 ± .8	1057.4 ± .8
	Over 30	1057.6 ±1.8	1052.8 ±1.2	1054.0 ±1.1	1052.3 ± 1.2

The measure of dispersion used in Table 5 is the standard error. Except for the years 1917–1919 the sex ratios by age for each parent differ by an amount which is statistically significant. Russell's data for the same country showed a similar relationship for fathers, but no consistent decline by five-year age groups of mothers.

The majority of the above studies suggest no explanation for the differences observed. The theory has been offered that the decline in masculinity according to advancing age is associated with increasing parity. This is merely sidestepping the issue as to what may be the causal factors of both phenomena. Concerning reproductive wastage, the same general comments apply here as in the discussion of parity. Parkes (1924b) reported a steady increase in the incidence of abortion and of stillbirth with increase in the age of the mother, concluding that this is responsible for the decline in sex ratio. If this is the case, the frequently observed decline with increasing age of the father must also be accounted for. The obvious suggestion is the correlation which exists between the ages of husbands and wives. Although data are almost entirely lacking on this point as concerns masculinity, a comprehensive report by Yerushalmy (1939) on stillbirths, sheds some light on the problem. This author found the following stillbirth rates per 1000 total births for the United States in the years 1931 to 1935.

Age of parent

	UNDER 20	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55 AND OVER
Father	40.2	33.1	28.8	30.0	35.7	43.2	48.8	52.3	52.3
Mother	38.4	30.4	29.9	35.1	45.9	60.2 (40 and over)			

These figures were based upon totals of 391,648 stillbirths and 10,590,775 live births. The data of Bertillon (1896) demonstrated a similar trend in the incidence of abortion with increasing ages of women.

The decline which occurs in the early

ages is not in conformity with the observations of Parkes on a much smaller body of data. In this respect the figures of Yerushalmy and Bertillon fail to account for any regular decrease in masculinity from the youngest to the oldest ages, but the higher stillbirth and abortion rates in the older age groups may be sufficient to account for differences in the sex ratio when a dichotomous division is made. Yerushalmy found that when each age of either parent was held constant, the stillbirth rates according to the ages of the other parent still showed the same U-shaped distribution. Since the stillbirth rates may change with the age of the father, quite independently of the age of the mother, this suggests that there may be changes with age in the quality of the spermatozoa. While it appears that changes in the incidence of reproductive wastage may account in part for the higher masculinity of progeny of both fathers and mothers who are over thirty years of age, irrespective of the correlation between their ages, it is by no means certain that the sex ratio of conception exerts no influence. Bonnier (1926) reported that fathers under 38 produced 51.30 percent male offspring, and those over 38 produced 53.06 percent males. He alleged that the causal factor in this case was an alteration in the primary sex ratio.

Goehlert (1882) observed that the sex ratio of foals was 89.8 from mares under 10 years of age and 93.9 from mares over 10 years. Stallions in the same age groups produced ratios of 77.9 and 105.5 for the young and old ages respectively. Johansson (1932) found only insignificant variations in the sex ratios of cattle with advancing ages of the parents.

King (1924) investigated the relation between age and sex ratio of rats, basing her conclusions on litter order rather than on age itself. Up to the fifth litter, which included parent rats from three to seven months old, the ratio was 103 males to 100 females. The proportion of males steadily increased through the tenth litter, or to parental ages of twelve or fourteen months. After this point there occurred a sudden drop to a sex ratio of 75 at the time when the rats were approaching the menopause. King maintained that physiological factors undoubtedly determine this change with the age of the mother, and suggested that a higher rate of mortality of very young foetuses may have caused the sudden drop at old ages. In another paper (1921) this author alleged, from a small number of cases, that very young rats (averaging 99 days) and old rats (over 15 months) cast litters containing a markedly higher proportion of stillbirths, than did mothers of intermediate ages. Since in rats, as in man, the sex ratio of stillbirths is higher than that of live births, the greater incidence of stillbirths among very young and very old mothers may partially account for the lower sex ratios in these groups.

TIME OF CONCEPTION

Another theory which fired biological interest was that of Thury (1864). He claimed that eggs fertilized shortly after ovulation resulted in female offspring, whereas later fertilization produced males. By thus regulating the time of service during heat, it was maintained, animal breeders could produce either sex at will. Thury cited 29 matings among cattle in which this was carried out with complete success. Clearly the paper had no statistical value. A number of equally inadequate reports on cattle were brought forth during succeeding years. The first figures of any magnitude were presented by Pearl and Parshley (1913). From a total of 480 calves, those resulting from conceptions early in heat had a sex ratio

of 98.4, in the middle of heat 115.5, and late in heat 154.8. It was quite obvious that Thury's original postulation as to the infallibility of the system of determination was false. Pearl's data supported the idea only insofar as it showed increases in the ratio of males to females. This author considered the problem to be of sufficient practical importance to merit further investigation. The subsequent study (Pearl, 1917a) on 1313 calves revealed that the sex ratio of offspring was unaffected by the time of service. The percentages of males were 51.0, 51.7 and 46.9 for the early, middle and late fertilizations, respectively. Apparently the earlier figures were based on insufficient data. The problem was further investigated by Cooley and Slonaker (1925) in the albino rat. These writers considered Pearl's study as inadequate in the method of determining the stage of the oestrous period, which was left largely up to the breeders. To overcome this difficulty a vaginal smear technique was used. In 72 copulations early in the period a sex ratio of 114.6 was obtained among the offspring. The late matings, numbering 65, yielded a ratio of 110.5. The difference was considered as insignificant.

In early studies Thury's hypothesis was brought forward as the cause of high sex ratios observed in Jewish statistics. Mosaic law forbids coitus within seven days after the cessation of the menstrual period. If conceptions early in the cycle resulted in a low proportion of males, and later conceptions in a high proportion, Jews should demonstrate a high ratio. This was put to a statistical test by Pearl and Salaman (1913). A small but accurate collection of data revealed a sex ratio of 105.4 in 415 births from parents who adhered strictly to the teachings of their religion. These authors discussed the reasons for believing that high sex ratios generally observed in Jewish births are due to faulty registration among these people. This view was upheld by Huntington (1938) in a recent investigation.

Siegel (1916, 1917) reported that conceptions which took place 1 to 9 days after menstruation resulted in 80 percent male children, 10 to 14 days in 43 percent, 15 to 23 days in 19 percent. The study was based on 180 births resulting from conceptions which reportedly took place when German soldiers were home on limited leaves of absence. Since it is difficult to see how the figures could be prejudiced by any lack of integrity of the German women during those trying times, the only justifiable criticism is in the scantiness of the data. In view of the difficulty of determining exactly when fertilization takes place, meager statistics appear to be an almost necessary attribute of studies on this problem. Fürst (1916) found 76 percent males resulting from 49 conceptions which allegedly occurred within the first four days after menstruation. The 144 conceptions during the remainder of the cycle gave rise to a sex ratio of 45 percent males. Pryll (1916) concluded that the proportion of males showed no significant variation with the time of fertilization. His figures were 55.1 percent, 52.7 percent, and 51.5 percent for early, middle, and late conceptions, respectively, in a total of 581 births.

It was at one time thought that changes in the sex ratio during the catamenial period were due to the degree of maturation of the ova at the time of fertilization. This view was prompted by the observations of Hertwig (1905) and others on the different proportions of sexes resulting from fresh and stale frog eggs. It is only within later years that light has been thrown on the relation between menstruation and the time of ovulation,

the findings indicating that the egg is shed generally about the fourteenth day. Further recent advances indicate that because of the short duration of life of the germ cells, the time of possible fertilization is limited to a day or two (Hartman, 1939). It is evident that the earlier writers had little or no basis for attributing human sex ratio variations to the degree of maturation of the ova.

Reed (1913) alleged that ova alternate between negative and positive phases, and fertilization merely checks this rhythm in one of the two periods. If it occurs in the negative period the offspring will be female, and if in the positive they will be male. These phases, it was claimed, are six hours in length, corresponding to the ebb and flow of tide in the particular locality. Reed argued that this was a carry-over from our aquatic ancestors which were active during the flow and inactive during the ebb. Accordingly, if one were to consult the tide table for his locality (or if it be an inland region, compute what the tide should be if there were a tide) he could produce either male or female progeny at will. There are several objections to the theory, the most obvious being how to account for multiple births of opposite sexes in man and other animals. Reed mentioned that his theory worked in a score or more of cases. King (1909) tested the theory in 651 toads, showing conclusively that it fails to hold for *Bufo lentiginosis*. Reed's theory, and others which claimed variations in sex ratio with time of conception during the sexual cycle, may be relegated to the class of untenable assertions.

SEASONAL VARIATIONS

The hypothesis that there are definite seasonal fluctuations in the proportions of the sexes dates back as far as Aristotle, who advised that the most propitious times of conception for obtaining female offspring were during the periods which now correspond to the dates September 22 to October 23, and January 21 to February 19. The thing which stamped his observation as pure fiction is that these conceptions must take place in the wane of the moon. Some recent investigators have reasoned that just as lower animals demonstrate definite breeding seasons, man and his domesticated beasts may in some way retain a remnant of this which finds expression in sex ratio variations. Others have investigated the possibility of seasonal changes in masculinity without offering any theory as to how they may have arisen. Parkes (1924c) showed that the sex ratio of the albino mouse was higher from March to June than from July to October, the earlier period being the time of greatest breeding activity. The same author (1926b) later found the highest sex ratio for the albino mouse in the months October–December (55.9 percent) and the lowest in April–June (48.2 percent) for 1772 births. In *Peromyscus*, Sumner, McDaniel, and Huestis (1922) reported the ratios 104.23 for February–April; 91.48 for May–July; 102.29 for August–October; and 85.21 for November–January. The two studies are in marked disagreement for the winter months. The report of King and Stotsenburg (1915) showed the high sex ratio of 117.7 for the period June–August, and the low ratio of 99.0 for March–May. The other two quarters each had a masculinity of 105.3, the total number of rats being 7619. The authors pointed out that the highest sex ratio was in the spring when the rats were at their greatest breeding activity, while the lowest proportion of males occurred at a time when the animals were not in their best physical condition. It was suggested that metabolic changes in

one or both parents might be the cause of the seasonal fluctuations, the metabolic changes in turn resulting from environmental variations. The results of Hanson and Sholes (1924) on the albino rat were antithetical to those of King and Stotsenburg, although the seasonal differences were insignificant. Curiously, all of the ratios showed an excess of female births.

According to Heape (1908b), records on dogs revealed a greater sex ratio during the winter than in the summer. He associated this with the low birth rate during the cold months. Whitney's (1939) study on German Shepherd dogs was in accord with the observations of Heape. In 1440 offspring the sex ratio was 116.1 for the period June to November, and 143.1 for the remaining months.

In an excellent investigation of the sex ratio of cattle, Johansson (1932) found no seasonal fluctuation for two breeds, despite a greater incidence of reproductive wastage during the summer months. In the cow the male foetus is more subject to stillbirth and abortion than is the female. Parkes (1926c) was unable to find any seasonal changes in the masculinity of 10,961 Duroc-Jersey pigs. The maximum and minimum quarterly ratios differed by only 1.7 percent. Over half of the births occurred in the March–May period. Further lack of seasonal variation was noted by Jull (1924) and by Lambert and Curtis (1929) in studies on the domestic fowl.

In man, Lewis and Lewis (1906) could detect no regular progression in masculinity which would suggest any definite seasonal change. Moreover, the maximum ratios for several groups of people occurred in different months, and likewise the minimum ratios. Bonnier's (1924) analysis of Swedish statistics gave results which were in accord with those of Lewis and Lewis. Ciocco (1938a) observed that the monthly oscillations in sex ratio during two quinquennial periods for the United States could easily be due to chance. The ratios in each period were based on totals of over ten million births.

The quarterly sex ratios found by Russell (1936) showed no significant differences for England and Wales, but were held to suggest that in the United States the summer months appeared to be more favorable for the production of male births.

Heape (1908a) presented data which purported to show that there are definite breeding seasons in Cuba, and that there are also seasonal fluctuations in sex ratio, the two being inversely related. The figures are presented in such a way as to make the association more apparent than real. Heape gave the months of highest and lowest birth rates and the sex ratios for these months, failing to list the sex ratios for the remaining periods of the year. He made six such comparisons, white and colored births for the years 1904, 1905, and 1906, which were intended to show high masculinities associated with low birth rate, and vice versa. One of these (colored, 1905) revealed the opposite situation. In another (whites, 1904) the total sex ratio for the year exceeded that for the month of lowest birth rate, showing that another month or months had an even greater ratio. In yet a third (colored, 1906) the total sex ratio for the year was lower than that for the month of highest birth rate, proving that another month or months had a still lower ratio. If, in Heape's study, there is actually an inverse relation between seasonal birth rate and seasonal sex ratio, he chose an unfortunate method of presenting the data. Bonnier also criticized Heape's analysis, and he demonstrated that in 2,316,321 Swedish births there was a seasonal fluctuation in birth rate but this was in no way related to mas-

culinity, the correlation coefficient being $-.020 \pm .07$. It was Huntington's (1938) belief that low sex ratio was associated with many births, both occurring during a season of good climate and high reproductive vigor of the parents. Although the discussion suffered not at all from any attempt at brevity of statement it failed to carry with it any great amount of statistical conviction.

It is quite certain that seasonal fluctuations in masculinity occur among mice and rats although the experiments are not always in accord. Among domesticated animals such variations are lacking. In man there is a diversity of opinion among students, but the greater amount of evidence indicates that there is no association between sex ratio and season. Parkes (1926c) concluded that those animals which have most marked breeding seasons likewise show seasonal oscillations in masculinity, and the greater the tendency toward domestication or artificial conditions the less will be the seasonal fluctuations in the sex ratio.

ANNUAL VARIATIONS

Little has been published on annual variations in sex ratios for animals lower than man, and what information does exist covers usually only two or three years. Jull's (1924) figures for the domestic fowl showed a slightly higher percent of male births for the period 1920–1921 than for the preceeding and succeeding two-year periods. The writer believed that this was due to the small number of parents in the middle group. Sumner, McDaniel, and Huestis (1922) found widely divergent sex ratios in *Peromyscus* over the period 1915–1921. Several of the differences were statistically significant. A study of mice by Parkes (1926b) showed no statistically significant annual fluctuations over a four-year period. In the albino rat King and Stotsenburg (1915) reported a sex ratio of 108.1 in 1914 and 106.9 in 1911–1913 in a total of 7619 offspring. Despite the large amount of data on sex ratios extracted from the herd books, little or no attention is given to yearly oscillations. There has been considerable diversity in sex ratios presented for domesticated animals by various writers whose data were based upon different years, but no significance can be attached to this in view of the non-conformity of breeds and other factors which may influence the proportions of the sexes.

Gini (1908) tabulated quinquennial sex ratios of human births for all European countries and a few in Africa, Asia, Oceania, and the Americas during the nineteenth century. Among the most complete of these, Norway, Sweden, Finland, and Spain showed gradually increasing ratios, while those of Scotland, England and Wales, France, Rumania, and British India were on the decline. Others remained relatively constant or else extended over too short a period to show any definite trend. The figures reported by Lewis and Lewis (1906) are in agreement with those of Gini. It was shown in this study that although a decline in masculinity might accompany a decline in birth rate as in the case of France and Scotland, the correlation did not always exist. Concerning this problem, Wolda (1927) concluded from his study of yearly changes in Sweden and Holland that a positive relationship existed between sex ratio and rate of conception. Russell's (1936) analysis of annual variations in sex ratios and birth rates for England, 1875–1932, revealed a a correlation coefficient of $+0.216 \pm 0.126$. When the data were divided according to the degree of urbanization, the coefficient was negative for London and for country boroughs, and positive for small urban

districts and for rural districts. All of these were statistically of less significance than the coefficient of correlation for the entire country.

Most of the investigations of annual fluctuations have been made with a view toward determining whether or not social upheavals influence the sex ratio. Düsing (1884) reported an increase in masculinity in Sweden following the war with Russia in 1789 and 1790. Lewis and Lewis were cautious about drawing any conclusions, but suggested that war recovery years might show an increased ratio, while recovery from epidemics might show a decreased proportion of male births. Gini believed that the rise in sex ratios during or shortly after wars was only coincidental. He found no visible trace of increase for the belligerent countries in the war of 1870 and 1871. The same author was unable to discover any significant annual variations as a result of a cholera epidemic in Italy, 1865–1866, or as a result of famine in India during the final years of the last century. Nixon (1916) studied the statistics of France, Prussia, England, Belgium, and Holland from 1863 to 1877 and agreed with Gini that there was no perceptible rise in masculinity shortly before, during, or after the Franco-Prussian War. He likewise found no significant increase or decrease in London, England and Wales, or Scotland in 1913, 1914 or 1915, as compared to previous years. The latter part of the investigation was too premature to justify any conclusion as to the effect of the World War. Mallet (1918), who had in addition the 1916 figures for the same countries, believed that the increase of the 1915 and 1916 ratios over those of the average for 1894 to 1913 could not be dismissed as accidental. The differences were not tested for significance. Similar observations, extended through 1918, led de Jastrzebski to agree with Mallet that the war raised the masculinity in the British Empire and also in Hungary, Finland, Switzerland, Denmark, and the Netherlands. Savorgnan (1921) observed that although there was a high sex ratio for England and for Germany during the war, it was even higher in 1919. He attributed the increase to a better condition of the mothers, which supposedly resulted from the infrequency of pregnancies during the war period. Ciocco (1938a) objected to this explanation on the grounds that the proportion of male births also increased in neutral countries where there was no prolonged absence of the husbands. Russell (1936) reported the mean sex ratios for twelve belligerent and seven neutral countries in the periods 1915–18, 1919–20, and 1921–23. For each nation the ratio of the middle period was highest. Mean numbers of males per 1000 females for all belligerent countries were in order, 1058, 1069, and 1058, and for neutral countries 1061, 1067, and 1062. A more detailed study by the same writer on England and Wales showed a much greater excess of males during 1919 than in the preceeding or following years. Russell suggested no reason for this rise during the demobilization period. For the United States, Ciocco presented annual sex ratios from 1915 to 1934. The data showed no fluctuations during war time which could not be attributed to chance. Ciocco further pointed out that there occurred no increase in masculinity coincident with the depression years following 1929.

An adequately supported theory has yet to be advanced to explain the increased sex ratios of European countries during and shortly after the World War. Curiously the increase occurred in spite of the higher incidence of reproductive wastage during those years. Bluhm (1921) stated

that among working women in Germany, 1915–1916, the rate increased to as high as 190 abortions for every 100 live births. Bayer (1938) reported that abortions increased during the war years in Germany by four to ten percent, but he believed these figures to be much too low. Inasmuch as the sex ratio increased for neutrals as well as for warring nations, the possibility suggests itself that a nutritional factor was at work. Bayer claimed that the German population which remained at home in communities of less than 2000 did not suffer the privations of the war. Nevertheless the sex ratio increased in such districts, a fact which he alleged resulted from conception by undernourished fathers who had returned from war duty. He believed that this supported the theory that inadequate diet is injurious to spermatozoa. The truth of the assumptions which Bayer made is clearly open to question.

NUTRITION

A few studies on the influence of nutrition have already been mentioned in the section concerning theories of sex determination. Since the food intake in man is not subject to control, allegations that the quantity of nutrition or quality of nutrition is responsible for human sex ratio differences have been mainly speculative. Düsing (1884, 1891) believed that undernourishment caused an increase in masculinity whereas plenty of food resulted in the opposite effect. Punnett (1904) recognized the limitations of studies on human populations. He found that the wealthier classes in London produced a higher proportion of male children than the poorer classes. This, he suggested, would indicate that better nourishment increased the sex ratio. However, after investigating the effect of other factors such as fertility and age of parents, which are also potentially related to sex ratio and economic classes, Punnett concluded that nutrition exerted little influence. Heape (1908a) upheld Düsing's claims against those of Punnett, although he contributed no statistical support from his own data. His belief that the influence of the male was negligible in problems concerning the sex ratio (he considered the female to be heterogametic) colored his thoughts on the subject of nutrition. Inasmuch as the "female-producing ova" suffer more from insufficient food than the "male-producing ova", according to Heape, the essential factor in nutrition is not merely the amount or quality of the food supplied to the mother, but her ability to transmit this nourishment to the ovary. We now know that if nutrition does influence the sex ratio its method of action must be somewhat different from this. So far as the nutrition of the female reproductive system is concerned, the effect, if any, would probably be one of differential elimination in the sexes of the embryos or foetuses. It is more likely that the quantity and quality of food supplied to the parents would influence the sex ratio only insofar as it affects the general health and vitality of those parents.

Probably because breeders hesitate to hazard the economic value of any considerable number of domesticated animals, the only available data on radical dietary changes deal with laboratory forms. Holmes (1910) starved *Drosophila ampelophila* for five generations and found no influence on the sex ratio of 4,733 offspring of the sixth generation. Further starvation up to the tenth generation yielded the same result, the ratio being approximately 90 females to 100 males in 9874 flies. Morgan (1911) mentioned that a long series of experiments on the addition of different sugars, salts, acids, and al-

kalies to the food failed to induce any alteration in the sex ratios of *Drosophila*. Schneider (1940) found that the sex ratios of progeny from thyroid-fed *Tribolium confusum* were not significantly different from those of the controls. The influence of nutrition on mice was investigated by Schultze (1904). Prolonged and rigorous restriction of the diet failed to produce variations sufficient to prove that the sex ratio had been altered by the privation, but the number of cases in the study was small. Sumner, McDaniel, and Huestis (1922) reported a masculinity of 104.31 ± 9.31 for 237 offspring of mice which had been fed an excess of proteins. The 228 births from mice which received normal rations had a sex ratio of 95.56 ± 8.61. The difference is far from being statistically significant. Slonaker and Card (1923) studied the sex of progeny from albino rats which had been on restricted diets. The sex ratios of offspring from five groups of undernourished parents were 84.0, 86.0, 90.0, 97.0, and 116.0. A total of 385 offspring of restricted feeders had a sex ratio of 95.5 as compared to 108.0 for 1001 births from matings of normal feeders. The writers concluded that the general effect of diet limitation was to decrease the proportion of male births. No explanation could be given for the excessively high rate of 116.0 in one group.

Parkes and Drummond (1925) reported a sex ratio of 44.6 ± 2.02 in the offspring of male rats which had received a diet containing two percent or less of yeast extract, and a ratio of 49.2 ± 1.96 in offspring of male parents which had received three percent or over of yeast extract. The difference relative to its probable error was 1.64. The writers considered this to be a small but significant difference. During the first eight weeks of the diet deficient in vitamin B the sex ratio of progeny was 49.1 ± 2.31 and during the next twelve weeks, 44.2 ± 2.04. The difference divided by its probable error was 1.6, and this again was considered significant. The writers maintained that a deficiency in vitamin B lowered the sex ratio in proportion to the duration of the deficiency. They attributed this effect to an alteration of the spermatozoa rather than to any difference in prenatal mortality. Although the figures tend to show the relationship claimed, the differences, when considered statistically, are not sufficient to warrant any definite conclusion. The same authors (1926) found that the offspring of rats which had been subjected to vitamin A deficiency had sex ratios that showed little variation from the normal.

Hoelzel, DaCosta, and Carlson (1939) reported the following sex ratios for crossmatings of rats which had been fed low protein (7.5–12 percent casein) and high protein (30–50 percent casein) diets.

TYPE OF MATING	NO. OF OFFSPRING	SEX RATIO
Low protein ♂ × high protein ♀..	420	107
High protein ♂ × low protein ♀..	553	145

The diets of the parents in the second group were later reversed, and the 455 progeny had a sex ratio of 92. The authors alleged from this that the better nourished parent had a tendency to determine the same sex in the offspring. The hypothesis was advanced that the state of protein metabolism induced in the well-nourished parent affects the germ cells and enhances the survival value of embryos of the same sex. The divergence of the sex ratios was thus ascribed to sex-differential resorption of embryos. Unfortunately, there is no record of the age of the parents, which itself may influence the ratio, although it was stated

that age deterioration had not set in. Contrary to the writers' statement, a diet high in protein content does not imply better nourishment. An excellent series of experiments by Slonaker (1939) showed that a diet of about 14.2 percent was most efficacious for the physiological functions of rats. The percentages of protein in five diets, otherwise the same, were I, 10.3; II, 14.2; III, 18.2; IV, 22.2; and V, 26.3. Diets I and II were definitely superior for all of the reproductive functions of the parents, showing the least amount of sterility, the greatest average number of young born, the youngest average age at birth of first litter, the oldest average age at birth of last litter, and the greatest reproductive span. As to the average life span, the order of diets with decreasing longevity was for males II, I, III, V, IV, and for females, I, III, II, V, IV. The sex ratios in a total of 7,933 births were I, 95.4; II, 103.6; III, 95.3; IV, 94.5; and V, 93.3. All of these ratios are below the normal for the rat. It is noteworthy that the diets which gave rise to the highest sex ratios were the same diets which resulted in the greatest reproductive efficiency and generally speaking, in the biologically fittest individuals, as indicated by length of life.

Experiments on mice and rats indicate, by and large, that nutrition of the parents probably does influence the proportion of sexes among the offspring, the better nourished parents producing a higher male sex ratio. Whether this results from an alteration of the ratio at conception, as postulated by Parkes and Drummond, or from a differential elimination of embryos, according to the views of Hoelzel, DaCosta, and Carlson, cannot easily be determined.

No experiments have been conducted which directly relate the vitality of parents to the sex ratio of births, but such a relationship has frequently been suggested as an explanation of other sex ratio variations. Thus King (1915) stated in connection with seasonal fluctuations in rats that the sex ratio was low when the parents were in poor physical condition, and high when in good condition. The same author (1921) found that stillbirths increased during times of low vitality. Parkes (1924a) observed that when conceptions in mice took place during a poor condition of the mothers there was a decrease in the sex ratio of the offspring, and, coincident with this, an increase in the amount of embryonic elimination. The poor condition, in this study, was the after-effect of a recent parturition. Savorgnan invoked this same principle to explain the increase in masculinity during wars. He alleged that due to the long interval between pregnancies the mothers were in better physical condition and the sex ratio was consequently increased. It is difficult to accept his hypothesis as to the fitness of the maternal organism in view of the rise in the rate of reproductive wastage during war years. If vitality of the parents actually does influence masculinity, it is much more plausible that sex ratio differences associated with age, nutrition, and social conditions are, at least in part, a resultant of this factor, either through alteration of the primary sex ratio or through differential rates of prenatal mortality.

FERTILITY

The large number of factors, environmental and biological, which influence fertility, complicate the study of any particular phase of this subject. The more important of these factors were comprehensively analyzed and discussed by Pearl (1939). In the following sections fertility will be considered only in rela-

tion to the biological variables concerned in the present investigation

Fertility and sex ratio

Amongst laboratory mammals, fertility is frequently referred to as litter size rather than as the number of offspring produced throughout life, or between certain time limits, as in the case of age specific fertility. Studies of the relationship between sex ratio and fertility in sub-human organisms are therefore not comparable to such investigations in man. Pearson found in his analysis of Weldon's (1906) data on mice that there was no individual relationship between litter size and sex ratio, but in a generation the correlation was .63 ± .17. Since Pearson believed that only environmental conditions could influence either sex ratio or litter size, he concluded that when these conditions favored large litters they also favored male births. In the deer-mouse *Peromyscus*, Sumner, McDaniel, and Huestis (1922) reported a sex ratio of 94.85 ± 2.94 in broods of one to three births, and 102.42 ± 3.01 in broods of four to nine births. No regularity of increase existed when litter sizes were taken separately, and the authors believed that the difference which resulted from grouping was accidental. Parkes (1924c) gave mouse sex ratios of 83.7, 120.5 and 125.4 for litters of size 1–4, 5–8, and 9–12 respectively. In a later study (1926b) this author found no correlation between litter size and the proportion of males in 1872 births of mice. The figures compiled by King and Stotsenburg (1915) for the albino rat revealed no definite change in sex ratio with increase in litter size. Wentworth (1914) reported no relationship between these variables in 174 litters of pigs, a finding supported by Parkes (1923). Wentworth also declared that multiple births of dogs gave sex ratios which differed but little from the expected proportions. By and large these studies indicate that among normally multiparous animals there is no association between sex ratio and litter size. The exception is in very small litters of mice, which probably in many cases represent the survivors from an originally much greater number of embryos. Since prenatal mortality in mice falls predominantly on males (Parkes, 1924a), this may explain the low masculinities sometimes observed in small litters. In his early study Parkes accepted this explanation, but abandoned it upon finding no abnormal sex ratio in small litters in his 1926 investigation.

Several studies have been mentioned concerning a possible relationship between the sex ratio and the birth rate, particularly in the sections on seasonal and annual variations. The terms "fertility" and "birth rate" were used interchangeably by Heape (1908a). It should be pointed out that an association between sex ratio and birth rate does not denote any association between sex ratio and fertility, except possibly for variations between decades or longer. Geissler (1889) found in statistics of nearly one million families that in sibships of seven and less the sex ratio was 105.8 while in sibships greater than seven it was 106.8. An analysis by Willoughby (1931) of families of university students revealed correlations between fertility and sex ratio of −.24 ± .08 for paternal sibships, +.07 ± .08 for maternal sibships, −.34 ± .07 for paternal cousin sibships, and −.12 ± .08 for maternal cousin sibships. In general his data tended to show an inverse relationship, but without further confirmation the result can hardly be considered conclusive. Winston (1932) reported ratios of 54.57 ± .53 percent in 1–2 child families, 52.74 ± .40 in 3–4 child families, and 51.48 ± .49 in families

of 5 and over, the total number of births being 15,763. The difference divided by the probable error of the difference was, for the first and third of these ratios, 4.28; for the first and second, 2.76; and for the second and third, 1.90. Punnett's (1904) data from *Burke's Peerage* showed this same relationship. The results obtained in these studies would be expected to follow from the decrease in sex ratio which is known to accompany an increase in parity.

Darwin (1871) suggested that declining races might generally exhibit high masculinity. The theory was investigated by Pitt-Rivers (1927) who found sex ratios much higher than normal at the ages of marriage in the declining populations of New Zealand, Australia, New Guinea, and Samoa. He believed that this was largely due to a higher rate of mortality among females than among males during childhood. This is contrary to all investigations on human populations. Powdermaker (1931) made genealogical studies of New Ireland and found that 55 percent of the births were males, whereas at the ages of marriage 53 percent were males. Obviously more males than females died before marriage age in this declining population, which indicates that the high masculinity found by Pitt-Rivers was probably due to a high masculinity at birth. It is difficult to account for this high sex ratio at birth, but it may be partially due to a lower fertility in declining races.

Fertility and longevity

Inquiries into the association between fertility and length of life were initiated by a desire to know if the burden of childbirth sapped the vitality of prolific mothers, causing them to die early, or if highly productive mothers had an inherently superior vitality and were therefore long-lived. Beeton, Yule, and Pearson (1900) investigated the relationship from English and American genealogical records. For American fathers and mothers the fertility increased throughout the entire age range, while for English parents it first increased and then decreased slightly after a length of life of about seventy years. Inasmuch as a positive association existed even beyond the age limit of reproduction, the authors concluded that "the peculiar physique in both men and women which leads to longevity is also associated with greater fecundity." The term "fecundity" obviously was meant as fertility. The weaknesses of this study were adequately discussed by Freeman (1935), the essence of the criticism being that the mean ages of death were not representative of the entire group, that the long time period of the genealogies permitted secular variations in fertility, which might influence the results, and that no account was taken of the duration of marriage.

Powys (1901) presented data from the vital statistics of New South Wales for the years 1898 and 1899. This study supported the claims of Beeton, Yule, and Pearson in that for women the fertility reached a maximum when the age of death was 65–70 years and dropped slightly thereafter. The figures were supplemented with statistics from 1900 to 1902 (Powys, 1905) giving a total of 83,362 births from 15,548 mothers. The trend in fertility of mothers was almost identical to that of the earlier analysis. Fertility increased quite regularly throughout the entire life range of 100 years for the fathers, but Powys cautioned against attaching too much significance to the increase because of the indefinite age of sexual decline in males. A number of very high correlation coefficients were reported, but these were computed from

averages rather than from individual family sizes. As in the preceding study no account was taken of the duration or age of marriage. Bell (1918) divided age at death of parents from the Hyde genealogy into the groups under 40, 40 to 60, 60 to 80, and 80 and over. The corresponding numbers of children per parent were 2.8, 6.0, 6.9, 7.1 for fathers and 3.4, 6.2, 6.6, 7.2 for mothers. This investigation suffered all the limitations of the work of Beeton, Yule, and Pearson.

Freeman (1935) attempted to overcome earlier difficulties by limiting the time periods covered in her genealogical data, and by taking age of marriage into consideration. Seventeen correlations were made between fertility of mothers and length of life beyond 45 years. Fourteen of these were positive. The relationship was most pronounced where the women had married under 20 years of age, the coefficient of correlation being $+.1027 \pm .0282$. Since this was probably not due to sampling error, it indicated that for women who married under 20 and died after 45 years of age, increased longevity was associated with greater production of offspring. While the remaining correlations were of lower order and could not be considered entirely beyond the range of sampling variation, they showed by and large the same associations. Dorn and McDowell (1939) using Australian statistics on 195,000 women, demonstrated the necessity of keeping year of marriage and age at marriage relatively constant. When these factors were controlled, the number of children per woman was found to increase with increasing length of life beyond 45 years. Unfortunately, due to the nature of the raw material, no valid statistical tests could be applied. In order to determine if the relationship held for women dying before the end of the reproductive period, Dorn and McDowell compared the mean number of offspring of women who died at each age under 45 years with the mean number of offspring of women who were married at the same age and produced during the same time period, but were still alive. It was found that, except for women who died within five years after marriage, the living mothers had produced a greater average number of children during the same period of time than the women who had died. The authors believed that although the data were not entirely satisfactory, they suggested that the positive association between fertility and longevity existed among women who died during the reproductive period as well as among those who died after this period.

Although the two most recent investigations failed to show as marked an association between the variables as did the earlier studies, they nevertheless upheld the general truth of the proposition. A rational explanation of the observed relationship is that the longevous individuals, and the most fertile, are from the biologically fittest part of the population. A series of studies in human longevity by Pearl (1934) indicated that long-lived individuals are constitutionally fitter than those who die at early ages. It is likewise quite certain that physical health is a factor in fertility. Brown, Greenwood, and Wood (1920) reported that among 632 women over 18 years of age who were classified according to health, those with excellent and good health produced an average of $2.09 \pm .05$ children while those with fair, poor, and bad health had produced $1.56 \pm .10$. These women had not yet reached the menopause. The mean ages at marriage and durations of marriages were almost identical for the two groups, but no indication was given as to the distributions.

Pearl (1936) classified physical health among the more important factors which influence fertility. Constitutional fitness and physical health are not synonymous, but in all probability they are closely related. The condition of the parents may influence the vigor and viability of the germ cells, or the rate of reproductive wastage, either of which is an important element in the expression of fertility.

SUMMARY

The numerous early theories and superstitions about sex determination have been almost completely abandoned as a result of more recent scientific investigations. Among the vertebrates and higher invertebrates sex determination is primarily a genetic process taking place at the time of conception, although hormonal or environmental influences can, in certain cases, dominate the chromosomal mechanism.

The weight of the evidence indicates that factors of an inheritable nature may give rise to transmissible variations from the normal sex ratio of any particular species. Just as there are interspecific variations in the normal sex ratio of subhuman organisms, there are also variations in masculinity among the different races and nationalities in man. Many studies have indicated that cross matings result in an increased sex ratio among the offspring of both man and lower animals, a result which may be an expression of the increased vigor which commonly accompanies hybridization.

By and large a high social or economic status is accompanied by a greater proportion of male births than is a low status, and rural districts demonstrate a higher masculinity than urban areas. The most widely accepted explanation for these differences is that high social and economic classes, and also rural inhabitants, have lower rates of reproductive wastage. Since in mammals abortions and stillbirths occur more frequently among male foetuses than among female, it has been asserted that populations with a low incidence of prenatal mortality will demonstrate a high sex ratio. This explanation assumes that there is little or no variation in the primary sex ratios of the groups under consideration, a postulate that is by no means valid for all cases. However, differential rates of reproductive wastage appear to be the most plausible explanation for variations in the sex ratios of socio-economic classes and of urban and rural groups. It likewise readily accounts for the higher sex ratio among legitimate births than among illegitimate births.

Although the relative ages of the father and mother exert no influence on the sex ratio of the progeny, it has been demonstrated that for man there is a decrease in the proportion of male offspring with an increase in the absolute age of either parent. Likewise the sex ratio decreases with increasing parity in human populations. Changes with age and parity in the rate of reproductive wastage probably account in part for the differences in sex ratio, but there may also be changes with age in the masculinity of conceptions.

Early reports that sex was determined, or the sex ratio influenced, by the time during the sexual cycle at which fertilization takes place have been disproved by more comprehensive investigations of the problem.

In lower animals, with a definite breeding season, there appear to be seasonal fluctuations in masculinity. There is little evidence that such is the case among domestic animals, and no conclusive statistical proof that seasonal

variations in sex ratios exist for man. Certain countries have demonstrated gradually increasing annual sex ratios over long periods of time, while others have shown decreases in the proportions of male births. Marked rises in masculinity occurred during and shortly after the World War among belligerent and non-belligerent European countries, but no completely adequate explanation for this has yet been offered.

The only controlled experiments on the influence which quantity or quality of nourishment has on the proportions of the sexes relate to experimental animals. The greater number of these investigations indicate that nutritional deficiencies result in a decreased sex ratio. Just how far these findings can be applied to human populations is a matter of conjecture. Attempts to analyze the problem of nutrition on the basis of economic class differences have proved ineffective.

There may be a slight decrease in sex ratio with increasing fertility in man, a result which would be expected in view of the decline in masculinity which accompanies increasing order of birth.

Among parents who died beyond the period of reproductive activity, the most longevous were found to have produced, on the average, a greater number of offspring than those who were short-lived. The relationship is not as marked as was alleged by the earliest investigators, but all studies have attested to its actuality. It is suggested that the parents who were long-lived were the most fertile by virtue of their being from the biologically fittest part of the population.

I wish to express my gratitude to the late Dr. Raymond Pearl for his many helpful suggestions in the preparation of this paper.

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CHEMICAL STRUCTURE OF THE RED BLOOD CELL

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THE highly differentiated, specific structure of the red blood cell is adapted to its main physiologic rôle. As Ponder (1934) has shown, the peculiar, biconcave shape of the erythrocyte appears to be most opportune from the standpoint of diffusion. It is a compromise between a sphere, which would offer the greatest carrying capacity with the smallest amount of structural material, and an infinitely thin disc, which would present the most favorable conditions for diffusion.

It would appear, therefore, that enclosure of the respiratory pigment in the peculiarly formed structure of the red blood cell allows the hemoglobin to function most efficiently. In addition, the striking differential permeability to ions which distinguishes the erythrocyte is an essential feature in the maintenance of a constant blood reaction. As so aptly stated by Barcroft (1921): "In the interior of the red blood corpuscle the hemoglobin exists in a world of its own; by this device nature has at a stroke increased the efficiency both of the blood and of the hemoglobin."

Since the time of Leeuwenhoek (1719) the actual structure of the mammalian erythrocyte has been a debatable question. His view held that the hemoglobin sol is surrounded by a membrane which gives rise to the so-called "ghosts" after hemolysis (Jorpes, 1932). Rollett (1870) introduced the term stroma to designate the sponge-like network, in the meshes of which he believed the hemoglobin solution to be held. The predominant view at present accepts the erythrocyte as a balloon-like structure, consisting of a semipermeable cell membrane or envelop of complex mosiac type enclosing hemoglobin, salts, and other substances in solution. The presence of a fine stroma network or a fluid cytoplasm capable of gelation under certain circumstances, is still speculative. However, as Ponder (1936) has stated, "The idea that the mammalian red cell is a bag filled with salts, pigment, and water, inert and dead or dying, is quite wrong, for it has a measurable metabolism of a rather complicated kind which is smaller than, but otherwise similar to, that of more fully organized cells."

SIMULTANEOUS COORDINATED STUDIES

When compared to the thoroughness with which hemoglobin has been investigated, our chemical understanding of the structural elements of the mammalian red corpuscle is surprisingly meager and inadequate. A thorough investigation of the structural elements of the red blood cell is complicated by the small amounts of this material which are so intimately combined with far greater quantities of hemoglobin (less than 10 per cent of the total solids of the whole cell is stroma;

up to 95 per cent is hemoglobin). Difficulties are encountered in separating, without appreciable loss or change in chemical composition, the framework of the cells from the associated hemoglobin and from contaminants; also in applying micromethods of sufficient scope and accuracy to permit differential studies of both the intact cells and stroma from a single sample of blood. This paper is a review of the results so far obtained from the coordinated studies which constitute the blood chemistry program of the Research Laboratory of the Children's Fund of Michigan; a program involving the cooperative efforts of numerous investigators and designed to obtain information about the chemical structure of the red blood cell. These integrated studies, covering a period of seven years and embracing observations on the physical and chemical characteristics of the blood in the normal individual (Erickson, Williams, Hummel and Macy, 1937a), in pathologic states (Erickson, Williams, Hummel, Lee and Macy, 1937b; Williams, Erickson, Bernstein, Hummel and Macy, 1937), and from investigation of practically hemoglobin - free posthemolytic residues prepared from various species (Beach, Erickson, Bernstein, Williams and Macy, 1939; Bernstein, Jones, Erickson, Williams, Avrin and Macy, 1938; Erickson, Williams, Bernstein, Avrin, Jones and Macy, 1938; Williams, Erickson, Avrin, Bernstein and Macy, 1938), have contributed pertinent information on the chemical nature of the red blood cell framework and serve as the basis for the discussion in the following pages.

An outline of the program is shown in Figure 1. This chart pictures the correlated physical measurements and chemical determinations utilized and emphasizes the importance of simultaneous observations on the same blood samples, as requirement for more complete understanding of the chemical structure of the erythrocyte.

The red cell in anemias

The various hemolytic and hypochromic anemias of the child and the hypochromic and pernicious anemias of the adult demonstrate extreme abnormalities in shape, size, weight and fragility of the erythrocyte which are characteristic of each type of anemia. A study of the chemical composition of the erythrocytes in these blood dyscrasias has pointed to the marked alterations which occur in their structural framework.

An outstanding change found in the anemic cell is the elevated cation content, a reflection of the increased sodium and potassium concentrations. The anion content of the corpuscles is usually found to be lower than normal. Maizels (1936) showed that the amount of base present in the normal erythrocyte is greater than that required to combine with cell chloride, bicarbonate, and hemoglobin. He suggested that the excess cations, found to range between 7 and 20 milliequivalents (meq.) per liter of cells, were in combination with some unknown anion (X^-) present in the corpuscle. Recently, Farmer and Maizels (1939) have shown that a part of the anion (X^-) is acid-soluble phosphates and glutathione.

A definite parallelism seems to exist between the excess corpuscular base and the undetermined weight of the cell calculated by subtracting the sum of the weights of corpuscular hemoglobin, water, total ions, and total lipids from the corpuscular weight. This undetermined weight probably consists chiefly of the structural protein, in addition to other substances such as non-protein nitrogen compounds, phosphoric esters, etc.

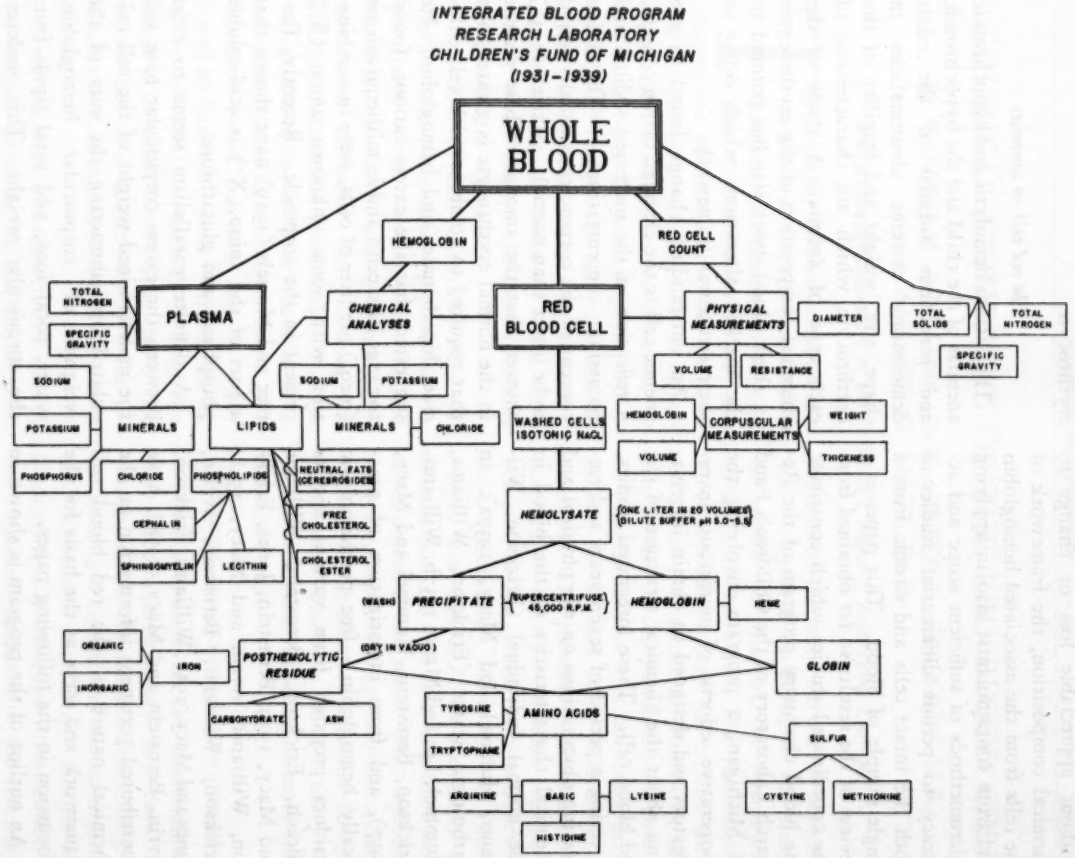
INTEGRATED BLOOD PROGRAM
RESEARCH LABORATORY
CHILDREN'S FUND OF MICHIGAN
(1931–1939)
WHOLE BLOOD
HEMOGLOBIN
RED CELL COUNT
PLASMA
CHEMICAL ANALYSES
RED BLOOD CELL
PHYSICAL MEASUREMENTS
DIAMETER
TOTAL SOLIDS
TOTAL NITROGEN
SPECIFIC GRAVITY
TOTAL NITROGEN
SPECIFIC GRAVITY
VOLUME
RESISTANCE
SODIUM
POTASSIUM
MINERALS
LIPIDS
SODIUM
POTASSIUM
MINERALS
CHLORIDE
WASHED CELLS ISOTONIC NaCl
HEMOGLOBIN
VOLUME
CORPUSCULAR MEASUREMENTS
WEIGHT
THICKNESS
PHOSPHORUS
CHLORIDE
PHOSPHOLIPIDE
NEUTRAL FATS (CEREBROSIDES)
CEPHALIN
FREE CHOLESTEROL
SPHINGOMYELIN
LECITHIN
CHOLESTEROL ESTER
HEMOLYSATE
(ONE LITER IN 20 VOLUMES DILUTE BUFFER pH 5.0–5.5)
(WASH)
PRECIPITATE
(SUPERCENTRIFUGE 45,000 R.P.M.)
HEMOGLOBIN
HEME
ORGANIC
INORGANIC
IRON
POSTHEMOLYTIC RESIDUE
(DRY IN VACUO)
GLOBIN
CARBOHYDRATE
ASH
TYROSINE
TRYPTOPHANE
AMINO ACIDS
SULFUR
ARGININE
BASIC
LYSINE
CYSTINE
METHIONINE
HISTIDINE

FIG. 1

The premise has been accepted that, in contrast to the plasma, the lipid composition of the erythrocyte remains relatively unchanged under a variety of conditions. In harmony with this accepted view the lipid composition of the red blood cells of normal children has been demonstrated to be not only relatively constant in amount and distribution, but similar in composition to the erythrocyte in adults. However, it has been found that, just as in plasma, certain definite changes occur in the red cell in the various types of anemia. Normally the various lipid constituents which make up the total lipid content of the red cell maintain a characteristic pattern, that is, approximately 58 per cent of the total is phospholipid, 23 per cent free cholesterol, 12 per cent neutral fat and 7 per cent cholesterol esters. Recently, seventy-five per cent of the neutral fat has been shown to be cerebrosides (Erickson, Souders, Shepherd, Teague and Williams, 1940). The outstanding changes in anemia are a lowered concentration in corpuscular phospholipid and a marked increase in cholesterol esters.

The physical and chemical abnormalities which the corpuscle exhibits in anemia are the result of a pathological process and may produce an impairment of the physiological activity or efficiency of the erythrocyte. On the other hand, it would not be unexpected to find the individual corpuscle more active because of the increased demands placed upon the reduced numbers to carry on the normal biochemical functions.

Bloor and his school (Bloor, Okey and Corner, 1930; Bloor and Snider, 1934; Boyd, 1935) have demonstrated that greater physiological activity in a tissue or organ is associated with increased amounts of phospholipid and free cholesterol; lowered physiological activity, degeneration, and retrogression are accompanied by decreased quantities of these particular lipids and augmented amounts of neutral fat and cholesterol esters. In this connection Gerard (1912) has suggested that higher potassium to sodium ratios in tissues are related to a higher state of activity. Also, it must be remembered that the term physiological activity (Bloor, Okey and Corner, 1930) is not confined to respiratory exchange but is meant to include all the physiologic processes of the living cell. Not only is the phospholipid decreased and the cholesterol esters increased in the anemic erythrocyte, but the potassium to sodium ratio is lower than in the normal.

If one accepts, therefore, the current views with regard to physiological activity, the lipid and mineral composition of the abnormal erythrocytes in the hereditary anemias of childhood and in pernicious anemia indicates that the cells are not only in a state of lowered function or activity, but also in a state of degeneration and retrogression. These results emphasize the view that the erythrocyte is an organized vital entity and not a hemoglobinized cellular corpse and that it experiences reactions such as lowered physiological activity, retrogression and degeneration similar to those of the more highly organized tissue cells in pathological conditions.

CHEMICAL COMPOSITION OF "POSTHEMOLYTIC RESIDUE"

Knowledge concerning the chemical nature of the structure of the erythrocyte has stimulated postulations involving both lipids and proteins. The importance of the lipids and minerals have been emphasized from the evidence secured through studies of the red blood corpuscle in the various types of anemia. Chemical studies on the protein phase of the cell

FIG. 1

framework, however, necessitate isolation of the structural material from the hemoglobin which comprises 90 to 95 per cent of the total red blood cell solids.

Methods for isolating the insoluble part of the erythrocyte which remains after hemolysis have been proposed by several investigators. A variety of terms such as red blood cell membrane, shadows, ghosts, or stroma have been suggested. It would seem preferable, however, until it can be determined whether the cell shadows or ghosts represent a membrane, an internal network or stroma, or a combination of both, that the material be designated by the term "posthemolytic residue".

Previous methods which have been proposed for the preparation of the posthemolytic residue have yielded products high in hemoglobin, and of altered composition due to the use of lipid solvents. After study and comparison of the former procedures, a method was finally developed in this Laboratory which yielded posthemolytic residues of consistent composition for similar samples of red blood cells and with comparatively little hemoglobin contamination.

Posthemolytic residues were prepared from the red cells of a number of species. Elementary analyses of these preparations are given in Table 1. The average hemoglobin content in all preparations was found to be 10 per cent or less, with the exception of the human residues. It can be seen that the posthemolytic residue is mainly a mixture of protein and lipid. The importance of this protein-lipid complex is shown by the fact that continuous extraction in a Soxhlet apparatus is required to liberate the lipids. Furthermore, vigorous washing procedures do not disturb the combination as the same sample followed through as many as five consecutive washings remained unaltered in composition. This demonstrated, also, that the lipids of the red blood cells are bound in the framework of the erythrocyte, for no significant amounts could be recovered from either the hemolysate or wash solutions. The latter is important, as analysis of the lipids permits a limited study of structural changes in the erythrocyte under various conditions without actual posthemolytic residue preparation.

Among the five mammalian species, beef, sheep, and horse posthemolytic residues are high in protein content and human is low (Table 1). The result is a low protein to lipid ratio for the first three species and approximately twice this for the human preparations. The nucleated avian red cell residues contain a comparatively small amount of lipid material and as a result the ratio of protein to lipid is quite high.

TABLE 1

Composition and lipid distribution of posthemolytic residues from various species

	BEEF	SHEEP	HORSE	HUMAN	AVIAN
	(*per cent of total residue*)				
Hemoglobin	5	2	10	23	5
Ash	3	3	2	5	2
Protein	57	68	53	50	89
Lipid	26	24	20	11	4
	(*per cent of total lipid*)				
Phospholipid	62	62	63	65	83
Free cholesterol	27	20	34	20	14
Cholesterol esters	3	0	2	4	0
Neutral fat	8	18	1	11	3
Protein:lipid ratio	2	3	3	5	22

Inasmuch as the residues from the various species demonstrate differences in lipid concentration, it is interesting to compare their concentrations to the lipid content and physical characteristics of the erythrocytes of the individual species

(Table 2). The lipid concentration per 100 grams of cells in the various species appears unrelated to the residue composition. The sheep cells have the highest concentration on the unit weight basis and the beef cells the lowest.

The erythrocytes of the various species, however, differ in their hematological and physical characteristics, cell hemoglobin content, volume, weight, and diameter. The red cells of sheep and beef are small, weighing approximately 41 and 61 micromicrograms, respectively, as compared to human and avian cells which weigh 93 and 130 micromicrograms, respectively. Because 100 grams of sheep cells contain approximately 2 to 3 times as many erythrocytes as the same weight of human and avian cells, analyses expressed on a unit weight basis obscure significant differences in composition of the various species' erythrocytes. When the lipid composition of the average single corpuscle of each species is calculated it is significant to see the order in which they are related. The beef and sheep red corpuscles, which show the highest concentration of lipids in the posthemolytic residue and are the smallest cells, have the lowest concentration of corpuscular lipid. On the other hand, the avian corpuscles, with the highest lipid concentration and largest dimensions, give a residue of very low lipid content. The human cells and residues are intermediate in their dimensions and lipid content. In the species studied, there appears to be an inverse relationship between the lipid content of the residue or framework of the cell and the corpuscular lipid concentration. It is now evident that the structural part of the red blood cell is a complex compound of, in the main, protein and lipid. What are the characteristics of these two phases from the standpoint of chemical makeup?

TABLE 2

Hematological and physical measurements of erythrocytes of different species

	BEEF	SHEEP	HUMAN	AVIAN
Red blood cells (millions per c.mm.)	8.4	10.7	5.2	3.0
Hemoglobin:				
Grams per 100 cc.	13.4	—	14.0	10.3
Per cell (micromicrograms)	16.3	11.0	29.0	34.4
Corpuscular measurements:				
Volume (cμ)	56.5	37.7	86.0	117.6
Weight (micromicrograms)	61.1	40.8	93.0	129.7
Diameter (μ)	5.9	5.3	7.7	12.1
Thickness (μ)	2.1	1.9	1.8	1.0
Diameter: thickness ratio	2.8	2.8	4.1	12.1
Total lipid (mg. per 100 gm.)	375	595	424	550
Total corpuscular lipids (mg. × 10^{-12} per cell)	222	243	394	726

Lipids in "posthemolytic residues"

Table 1 also presents the results of differential lipid analyses of the posthemolytic residues of the various species. Phospholipid and free cholesterol make up the major portion of the total lipid. The mammalian residues are quite comparable in their phospholipid makeup, which comprises a little over 60 per cent of the total lipid. Avian residue is much higher in phospholipid with a concomitant decrease in the other fractions. The lipid distribution of these residues is quite similar to that of the corresponding erythrocytes.

Recently, with the aid of newly developed methods, we have demonstrated that cephalin is normally the major component of the phospholipids of the red blood cell and posthemolytic residue. The remaining portion is divided about

equally between lecithin and sphingomyelin. Furthermore, the lipid constituent of the erythrocyte and residue which has been called neutral fat appears to consist largely of cerebrosides. Inasmuch as cephalin, which is found to be the predominant cellular phospholipid, is considered to be the structural type of phospholipid whereas lecithin is designated as the metabolic type (Sinclair, 1934), these observations are additional evidence to that previously given, that the lipids of the red cell are, normally, bound in the framework.

The fact that phospholipid and free cholesterol comprise practically all of the total lipid of the posthemolytic residue not only identifies this substance as protoplasmic in nature but supports the view that the red cell is a living entity. The French school of workers, Mayer, Schaeffer and Terroine, have demonstrated, and Bloor and his school, in this country, have confirmed the essential rôle that phospholipids and cholesterol play in the vital economy of living cells (Bloor, 1928; Bloor, Okey and Corner, 1930). These particular lipids are found to be present in constant amounts in living tissue irrespective of the state of nutrition. They are designated as the *element constant* and considered to be constant and integral components of protoplasm.

Protein in "posthemolytic residues"

The next consideration is the protein moiety which makes up two-thirds to three-fourths of the structural entity of the red cell. Seven amino acids have been determined in the lipid extracted posthemolytic residues of five different species and results of these analyses are shown in Table 3. It is evident that the residue proteins of the various species are similar with respect to the content of amino acids here determined. The cystine and methionine contents account for 85 to 95 per cent of the total sulphur and show the greatest variations among the different species. The values for histidine, arginine, tyrosine and tryptophane are slightly lower, but otherwise similar to the values obtained by Jorpes on his preparations. The seven amino acids thus far determined account for 25 to 27 per cent of the total protein nitrogen.

TABLE 3

Nitrogen of amino acids expressed as per cent of total nitrogen of the posthemolytic residue

	BEEF	SHEEP	HORSE	HOG	HUMAN
Histidine	4.0	4.7	4.2	4.5	4.3
Arginine	12.0	11.4	11.6	11.4	11.8
Lysine	4.8	4.5	5.4	5.1	5.8
Tyrosine	1.6	1.4	1.6	1.6	1.7
Tryptophane	1.2	1.0	1.1	1.3	1.2
Cystine	0.7	0.8	0.8	1.0	0.8
Methionine	1.2	1.6	1.6	1.5	1.4
Per cent nitrogen accounted for	25.5	25.4	26.3	26.4	27.0

TABLE 4

Per cent average basic amino acid composition of lipid-extracted posthemolytic residue, compared with values in the literature for serum orosin, hemoglobin and cattle fibrin

	HISTIDINE	ARGININE	LYSINE
Posthemolytic residue	2.1	5.0	3.6
Serum orosin	2.1	4.7	6.9
Hemoglobin	7.5	3.3	7.9
Cattle fibrin	2.5	7.7	10.1

On the basis of these determinations the proteins of the various residues appear to be quite similar. The content of basic amino acids indicates that the protein moiety of the red cell framework is acidic in nature as stated by Jorpes (1932). Table 4 presents a comparison of the basic amino acid composition of the posthemolytic residue protein and other blood

proteins. The distribution of the basic amino acids in the posthemolytic residue preparations is obviously different from that of hemoglobin and fibrin. It is similar in composition to Block's serum orosin with respect to histidine and arginine. The serum orosin, however, contains approximately twice as much lysine as the posthemolytic residue protein. Not only are the proteins of the posthemolytic residues different in composition from the other known blood proteins but they are also characteristic substances themselves. While at present there is no assurance that the protein moiety of the erythrocyte residue is composed of a single protein, it seems certain that if there are two or more individual proteins present, they appear in the erythrocyte framework of all these various species in the same proportions.

Erythrocytes in a controlled anemia

Finally, in an effort to study directly the effect of a pathological condition on the chemical composition of the erythrocyte framework, posthemolytic residue was prepared from red cells in an experimental anemia which is known to affect the hematological (Gruhzit, 1931), physical and chemical characteristics of the corpuscle (Williams, Erickson, Black and Macy, 1941). Dogs were maintained in a moderate to severe state of anemia with normal propyl disulphide for a period of three months. At the end of this time they were exsanguinated and posthemolytic residue was prepared from the combined washed erythrocytes of the animals. Another residue preparation was made from the blood of normal dogs.

Fractional lipid and protein analyses of the anemic dog blood material showed a content of 71 per cent protein and 12 per cent lipid, making the protein:lipid ratio 5.9. In the normal dog blood material the protein constituted 57 per cent, the lipid 17 per cent, a protein:lipid ratio of 3.4 (unpublished data). It can be seen that the protein to lipid ratio was increased by the anemia. This is significant in the light of the changed dimensions of the erythrocytes as the result of the anemia. The anemic erythrocytes were larger and heavier, more like the pernicious anemia red cell. In addition, the total base of the cell was augmented, indicating an increase in the anion (X^-) similar to that found in the human anemias had occurred.

The quantity of red cell framework

The question may be asked: How much of the cell is taken up by the fixed framework? No attempt has been made to secure a quantitative yield of residue from the red cells, as it was essential to reduce the hemoglobin content of the product to a minimum with vigorous washing procedures. It is possible, however, to calculate the quantity of fixed framework, if we assume that all of the cell phospholipid is bound as such. Evidence has been given that such an assumption is relatively safe with respect to the normal erythrocyte.

Table 5 shows how this calculation may be made. The results of the calculation of the per cent of structural material of the red blood cell of various species are presented together with the values obtained for rabbit erythrocytes by Fricke, Parker and Ponder (1939) using the conductivity method. The erythrocytes of the various mammalian species have from 1 to 4 per cent structural material. The large nucleated avian cell has a much larger amount, 13 per cent. The amount of structural material is relatively increased in the anemic cell and may be related to a number of factors. It will be recalled that the residue protein is acidic in nature and that in the anemias

the elevated base tends to parallel the undetermined cell weight.

TABLE 5

Calculation of fixed framework of erythrocytes of various species

$$\frac{\text{Weight of phospholipid per average cell}}{\text{Per cent phospholipid in residue}} = \text{Weight of residue per average cell}$$

$$\frac{\text{Weight of residue}}{\text{Weight of average cell}} \times 100 = \text{Per cent of fixed framework in an average red blood cell}$$

	PER CENT
*Rabbit	2.3
Beef	1.3
Sheep	2.3
Human	3.3
Avian	13.3
Dog(normal)	4.2
Dog(anemic)	6.7

* From Fricke, Parker, and Ponder. 1939.

SUMMARY

It appears, from the evidence at hand, that the red blood cell is a definitely organized complex. The hemoglobin and salts are held in a composite structure, the essential components of which are protein and lipid. The different hematological and physical characteristics of the erythrocytes obtained from various species seem to be associated with the quantity relationship of the protein-lipid complex of the red cell framework. The fact that the protein and lipid patterns are remarkably similar in the different species and that in certain pathological states the lipid pattern, at least, undergoes marked alteration points to the red blood cell as a vital entity similar to other tissue cells which likewise undergo chemical changes under pathological circumstances.

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PULSE RATES

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WHEN the *Medical Impairment Study* of 1929 (1) was in course of preparation, during 1931, it seemed desirable to preface the investigation of abnormal pulse by a review of some of the characteristics of normal pulse. As both time and space were limited, the matter was summarized in a brief reference to the average pulse rates for men (74.7) and women (75.8), derived from 30,000 insured lives resident in the United States and Canada. It may be of interest to review here at this time some of the other matters dealing with pulse rates, which were brought together in connection with this medical research.

The pulse rate in human beings was observed three or four centuries before the Christian era, but not until Harvey, in 1614, discovered the fact that the blood circulates through the body, was there any incentive to record the rate at which the heart does its pumping. Early in the following century the pulse watch was invented, but for another hundred years little of permanent value was recorded scientifically regarding pulse rates.

Upon surveying the literature of the subject—and definite facts were not easily accessible—it appears that the classic paper was that of Dr. William A. Guy (19). His observations were made on healthy men who had not eaten food for two hours previously; they were made between noon and 2 P.M. before any violent exercise or excitement. The pulse was taken first in the erect, then in the sitting and then in the recumbent posture. In the sitting posture the back was unsupported; in the recumbent position the head was slightly raised. A short interval was allowed to elapse after each change in posture. One hundred such cases were examined, aged from 20 to 50 and averaging 27 years. The extreme cases were found to differ considerably from the mean readings; thus between standing and lying the difference ranged from 44 to 4 pulsations per minute. Two general rules were deduced,—first, a change from the erect to the sitting, from the sitting to the recumbent or from the erect to the recumbent position, lessens the frequency of the pulse; second, the difference between pulse when standing and sitting is greater than between sitting and lying. After all exceptions to these rules had been excluded, there were 66 experiments which gave the following average readings for men at mean age 27:—

standing 81.0; sitting 71.1; lying 65.6.

Similar procedure for 50 women gave the following readings:—

standing 91.3; sitting 84.4; lying 79.7.

Dr. Guy stated his belief that muscular contraction, whether employed to change

the position of the body or to maintain it in the same position, accelerates the pulse. He showed that taking a meal may normally increase the pulse rate of a healthy person by from ten to twenty beats per minute; and that the pulse rate in the evening differs from that of the morning by about the same amount.

For many years the textbooks and encyclopedias have given 72 as the average pulse rate per minute for men, and 80 for women. This results in an excess of eight per minute for women as contrasted with only one per minute in the 30,000 lives referred to above. It has been suggested that the relatively lower pulse rates for women today may be due to (1) the more athletic, out-door life of modern times and (2) absence of the nervousness which was doubtless often present on examination a century ago.

Mary Hallock (23) has contributed an interesting discussion on pulse and rhythm. The close connection between pulse and rhythm has been speculated upon since the fourth century B.C.—it will be recalled that the circulation of the blood was not discovered until shortly before Shakespeare's death. Seventeen bandmasters have reported a "beat" of 64 to 72 per minute; this was what soldiers had found it agreeable to march to. On the other hand, it has been noted that the favorite metronome of the composer Chopin was 88 per minute. He was of a nervous, tubercular nature and his own pulse rate was probably not far different from this figure. "Perhaps the arm of knowledge can thus measure the living pulse of those who have long since died."

For many years it has been known that the larger animals have a slow pulse rate, and vice versa. Thus the elephant, 25–28; horse and ox, 36–50; sheep, 60–80; dog, 100–120; rabbit, 150; mouse, 700 and canary bird, 1,000. However the heart and the heart-beat have no fixed relation to the size of the animal. The amount of driving work done is a principal factor. In a fish the heart need pump blood only to the gills; its weight is only $\frac{1}{10}$ of 1 per cent of the weight of the fish. In birds, on the other hand, there is a large amount of work to be done; their hearts are from 1 to 3 per cent of body-weight. The pigeon's heart weighs as much as that of a salmon fifteen times its size.

Warm-blooded animals must maintain a temperature higher than that of the surrounding air. They are constantly giving off heat to the surrounding environment and this makes more work for the heart. The smaller the animal, the larger is the *relative* area over which to give off heat. If the animal with the larger *relative* area cannot pump more oxygen-containing blood with each beat, its heart must pulsate more frequently. Thus the heart rate varies not only according to the size of the heart, but also in accordance with metabolism. The pulse rate seems to vary directly with the carbon dioxide output. Thus we have the ratios

$$\frac{\text{Mice}}{\text{Men}} = \frac{\text{CARBON-DIOXIDE OUTPUT } 8.4}{0.6} = \frac{\text{PULSE RATE } 1008}{72}$$

A series of observations for various birds and animals made by Florence Buchanan, (8) have verified this simple relationship.

The fact that the area of small objects is relatively greater in ratio to volume than the area of large objects of the same shape may be readily shown by noting area and volume for two cubes of different sizes. The area depends on the square of the diameter and the volume on the cube of the diameter; hence a reduction in diameter leads to an *increase* in the ratio of area to volume. By a similar con-

sideration of rectangular solids it may be illustrated that tall, slender people have greater surface area than short, rotund persons of the same weight. This gives a clue as to why the former tend to have a higher pulse rate than the latter. In the first example we have changed the size but not the shape; in the second example, the shape but not the weight.

The domestic animals such as the ox and the chicken usually have slow hearts and have been chosen by man so as to fatten easily. The race-horse and the deer represent the opposite tendency and have large hearts. The pulse rate among human beings doubtless varies according to the driving power exerted and the basal metabolism as well as the force of the central nervous system.

TABLE 1

Pulse rates and age (after Tigerstedt)

AGE	AVERAGE PULSE	MAXIMUM	MINIMUM
0	134	160	101
1	111	136	84
2	108	134	84
3	106	124	80
4	103	133	80
5–9	96	128	68
10–14	87	120	56
15–19	79	112	52
20–24	74	100	41
25–44	71	104	50
45–49	72	100	49
50–59	73	108	48
60–69	74	100	52
70–79	75	104	50
80 and over	77	98	63

The pulse rate varies greatly in health according to age, sex, temperament, exercise or rest, emotional states, temperature, time of day, posture, atmospheric pressure and personal idiosyncrasy. During the pre-natal period the average number of pulsations per minute is about 150. The figures in Table 1 have been derived from 2,500 examinations reported by Dr. Robert Tigerstedt (44), of the University of Helsingfors, Finland.

These figures have been represented graphically and the results are shown in Figure 1. The higher pulse rate in children is due in part to the smaller size of the body, i.e., large area in relation to volume, but is also related to the more active metabolism of the small and young individuals. It is evident that the pulse rate decreases as age advances until about age 25. In the above data there is a slight increase in the pulse rate after age 45, but other observers have noted either the opposite tendency or no change with advance in age. Quite clearly other factors than age are of distinct importance, since in a number of age-groups the maximum reading of pulse was at least twice the minimum.

In passing, it may be noted that the decreasing pulse rate during infancy and youth is accompanied by a corresponding increase in blood pressure. This inverse relationship does not usually continue into the later years of life.

The higher average pulse rate for women than for men is doubtless to be associated with their smaller average physique.

Dr. James Mackenzie (34) points out that the healthy heart is readily affected by the most trivial circumstances. Thus the exposure of the body to cold air slows the pulse by a few beats per minute. The addition of clothing by increasing warmth increases pulse rate. In slow walking a pulse of 68 may go up to 100, in quick walking to 140 and in running to 150 per minute. The pulse rate is usually more rapid in the evening than in the morning and is also higher after a meal, probably largely as a result of the addition of heat to the body through

food. Drinking hot water accelerates and drinking cold water retards the pulse.

In the year 1931 three large life insurance companies, the Metropolitan, New York Life and Equitable, contributed pulse rate data for 36,000 men and women who applied for life insurance. Of these 18 per cent were women. The averages

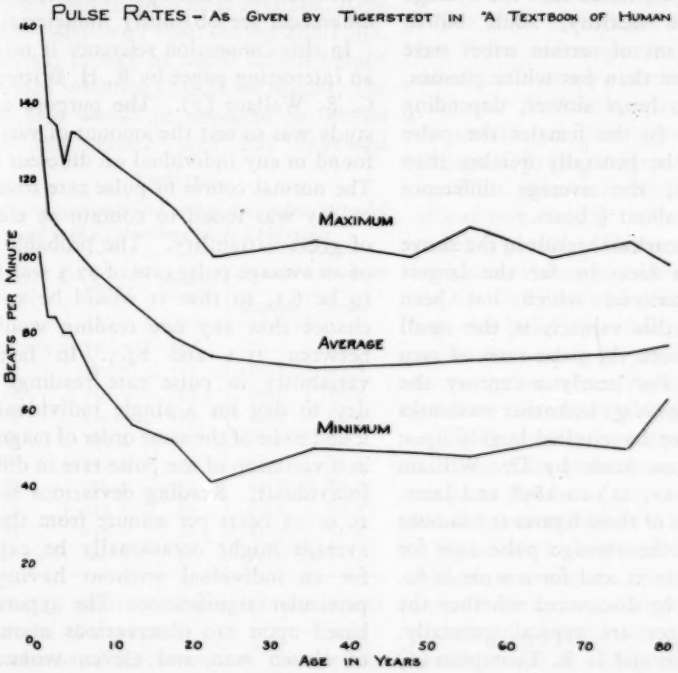

FIG. 1

were obtained in quinary age-groups from 10 to 70. These averages differ considerably from those which have been generally accepted by medical men throughout the world as representing pulse rates among the races of Northern Europe. The average found for white men was 74.7 and for white women 75.8 per minute. At age-group 10–14 the average for males was 3.5 greater, and in the age-group 64–70, 2.5 less than for all ages. Similar age differences hold for females. At ages 15–63 the pulse rate for each sex was practically constant. The excess of the pulse rate for women over that of men ranged from .6 to 1.9 and averaged 1.1. It may be regarded as practically constant for all ages from 10 to 70.

One of the companies gave pulse rates for substandard (i.e., under average) lives and also for Negroes. No significant difference could be discovered between the pulse rates of whites and Negroes nor between pulse rates of standard and substandard lives. This set of statistics is doubly interesting, because a few early observers such as Jousset

had recorded that African lives tended to have higher pulse rates than whites. That may have been due, however, to (a) the more primitive life of the natives, (b) a tropical environment, and to some nervousness upon having the pulse readings taken.

With respect to the pulse rates of different races, it is interesting to note that Dr. Hrdlička (26) found that the average pulse rates in healthy, adult North American Indians of certain tribes were decidedly lower than for white persons, being 5 to 20 beats slower, depending on the tribe. In the females the pulse was found to be generally quicker than in the males, the average difference amounting to about 6 beats.

The most remarkable result in the above figures, which form by far the largest volume of material which has been published on this subject, is the small difference between the pulse rates of men and women. For nearly a century the *Cyclopedia of Physiology* and other textbooks for medical men have relied largely upon the observations made by Dr. William Guy (19, 20, 21, 22) in 1838 and later. Upon the basis of these figures it has been accepted that the average pulse rate for an adult man is 72 and for a woman 80. It remains to be discovered whether the present averages are typical generally.

R. H. Britten and L. R. Thompson (6) obtained an average pulse rate of 81.1 for men in the garment, glass, pottery, foundry, steel, chemical, cigar, gas and cement industries. The authors stated, however, that this relatively high rate may have been largely due to the high proportion of foreignborn who from ignorance were nervous at time of examination. The rates of pulse for women were about two beats per minute higher than for men.

The question whether pulse rates vary according to season, such as between winter and summer, has been touched upon in the literature, but without any substantial statistical basis. One life insurance company contributed in 1931 observations on the lives of 16,000 men and found average pulse rates of 74.8 in December, 74.6 in March, and 74.2 in July. These readings differ by only a fraction of a beat per minute, and the differences are obviously immaterial.

In this connection reference is made to an interesting paper by R. H. Britten and C. R. Wallace (7). The purpose of the study was to test the amount of variation found in any individual on different days. The normal course of pulse rate from day to day was found to contain an element of great variability. The probable error of an average pulse rate of 77.3 was found to be 6.2, so that it would be an even chance that any one reading would lie between 71.1 and 83.5. In fact the variability in pulse rate readings from day to day for a single individual was found to be of the same order of magnitude as a variation of the pulse rate in different individuals. Reading deviations such as 10 or 15 beats per minute from the true average might occasionally be expected for an individual without having any particular significance. The report was based upon 110 observations upon each of eleven men and eleven women.

There is very little published information as to the relation of pulse rate to height and weight. In 1926, R. H. Britten and L. R. Thompson (6) stated that for the same height the average pulse rate seemed to decrease slightly as weight increased. For an increase of 35 pounds there was a decrease of 2.5 beats.

Volkmann (in Howell (25), p. 587) is quoted as saying "Pulse rates vary inversely as the five-ninth power of the

height." If this were true the following would be typical figures.

Height.....	4'0"	4'6"	5'0"	5'6"	6'0"	6'6"
Pulse rate...	90	84	79	75	71	68

This rapid *decrease* in pulse rate as height increases does not appear to have been substantiated statistically. Some tendency in that direction might be expected if persons of all ages were included, since children, before growth is attained, have higher pulse rates than during adult life. However, it may be noted that if height increased without change in weight, the pulse rate would be expected to *increase* since the surface area has increased without a greater volume, as was noted earlier in this paper.

TABLE 2

Height, average weight and average pulse rates (ungraded and graded) of soldiers in the Philippines

HEIGHT (*inches*)	AVERAGE WEIGHT (*lbs.*)	NUMBER OF MEN	AVERAGE PULSE RATES	
			Ungraded	Graded
63 or less	132.9	27	79.0	79.3
64	134.7	114	80.3	79.8
65	139.9	157	80.0	80.4
66	141.9	187	81.5	80.9
67	144.4	225	80.4	81.2
68	147.2	251	82.3	81.6
69	152.7	127	82.6	81.7
70	155.6	80	80.2	81.7
71	159.0	40	81.5	81.9
72 and over	164.5	35	83.0	82.3

In the usual circumstances the weight of the individual increases with his height, and average pulse rates have been obtained for a group of healthy soldiers in the Philippines arranged in this manner (see Chamberlain (10)). The average age of the 1,243 men was 27. The final column in the foregoing table has been added by grading the pulse rates in accordance with Henderson (see Bowerman (5)). It shows upon the average an *increase* in pulse rate of one beat per minute for each three inches of increase in height.

The above figures appear to be the only ones which have been published on this subject. It should be observed that they do not accord with the hypothesis that the average pulse rate is generally lower among those of larger physique. The observations were made in the tropics and the average pulse rate, 81, was six beats greater than that of the much larger group of insured men reported above.

The effect of exercise upon pulse rates has been investigated in considerable detail in Germany, particularly by Christ (11) and F. Tewildt (43). More clear-cut conclusions were given, however, by George Kolb (32). He made observations upon the pulse rates and blood pressures of oarsmen during and at the end of a training period of four weeks. His chief conclusions were as follows:

(A) Under normal conditions of training the pulse rate fell during the four weeks from 70 to 80 per minute to about 60 to 45 beats per minute. The blood pressure increased to 160 to 185 mm.

(B) In cases of overtraining and lost condition the pulse rate increased again to 90 to 100 beats per minute while the blood pressure fell to 120 mm.

Signs of a healthy development in athletic training are therefore a slow action of the heart, a high blood pressure and a strong pulse. Signs of an unhealthy condition are an accelerated action of the heart, a low blood pressure and a weak pulse.

Associated with the results of exercise are the milder effects of a change in posture. A number of investigators from the days of William Guy to the present have noted the changes in pulse rate which follow a change as from recumbency to a standing posture. One of the most carefully executed papers was based upon pulse rate readings of aviators in the Medical Research Laboratory and

School for Flight Surgeons at Mitchell Field, Long Island (Schneider and Truesdell (39)). The following table summarizes their data regarding postural change among 2,000 men of average age 25.

TABLE 3

Postural pulse rate change shown by the median

PULSE RATE IN RECUMBENCY	INCREASE ON STANDING		PULSE RATE STANDING	DECREASE ON RECLINING	
	104 men physically fit	2000 men unselected		104 men physically fit	2000 men unselected
60	18	21	60	—	3
63	15	21	63	3	3
66	15	18	66	3	6
69	15	18	69	3	6
72	12	18	72	6	9
75	12	18	75	9	9
78	12	18	78	9	12
81	9	15	81	12	12
84	6	15	84	15	15
87	6	15	87	15	15
90	—	15	90	15	18
93	—	12	93	15	18
—	—	—	96	15	21
—	—	—	99	18	21
—	—	—	102	—	21
—	—	—	105	—	24

Since the pulse rate varies according to the vigor of the body and the rate of metabolism, it may be expected to vary also with respect to occupation. To throw some light upon this question, a large life insurance company has contributed pulse rates of 7,060 men arranged in broad occupational classes as shown in table 4.

The average pulse rate of the entire group was 74.7.

The men in the above table were all granted life insurance at standard rates and at ages 20 to 65 only. The tendency to a lower pulse rate among those engaged in outdoor activity may be noted, and more particularly among those in the heavier types of muscular work.

TABLE 4

Pulse data by occupational classes

OCCUPATION	NUMBER OF CASES	EXCESS OF PULSE RATE OVER AVERAGE OF ALL CASES
Agriculture	604	−2.0
Extraction of minerals	80	−1.1
Manufacturing	981	+.2
Construction	535	+.2
Skilled occupations, n.e.c.	230	−.2
Transportation	763	−.5
Trade	2,064	+.4
Public service	72	−.3
Professional service	772	0
Domestic and personal service	354	+.5
Clerical occupations	475	+1.0
Non-gainful occupations	130	−.6
	7,060	0
Light work	4,620	+.3
Heavy work	2,440	−.6

The possible significance of pulse rates in relation to disease has been touched upon by several investigators. Probably the layman would associate a rapid pulse with a nervous temperament and tubercular tendencies; while a slow pulse would accord with a phlegmatic tone. Dr. Eugene F. Russell (38), in 1924, stated that a pulse persistently above 90 would usually indicate some organic trouble such as tuberculosis, toxic goitre, nephritis, or possibly the excessive use of alcohol, tobacco or other stimulants. Paroxysmal tachycardia is also occasionally found under these conditions. "A slow pulse such as 30 to 50 may indicate heart block, myocarditis, aortic stenosis, some change in the blood vessel wall or cerebral tumor."

On the other hand, J. J. Walsh (45) stated in 1900 that "the most frequent cause of slow pulse is gastro-intestinal disturbance including cancer of the digestive tract." He cited the case of a woman aged 31 with a pulse rate of 36, who was

nervous and high strung and not at all phlegmatic.

Dr. Florence Buchanan (8) thought that rapid pulse is more indicative of nervous and respiratory troubles than of circulatory impairments. This, I believe, is the prevailing opinion.

In the life insurance practice of the United States and Canada, a pulse rate between 55 and 90 per minute is generally looked upon as being acceptable at standard rates of premium.

In the *Medical Impairment Study* (1) it was found that the mortality on cases with a persistent pulse of between 90 and 100 beats per minute was about 150 per cent of the standard. A small group of cases accepted with a persistent pulse of over 100 beats per minute indicated about the same percentage of standard mortality, but the selection was probably severe on these latter cases.

The mortality under policies with a persistent pulse rate between 55 and 65 beats per minute was about 80 per cent of the standard. Apparently a moderately slow pulse is a good factor as far as mortality is concerned. Cases with persistent intermittent pulses and those with irregular pulses showed a higher than average mortality.

SUMMARY

When the *Medical Impairment Study* (1929) was in course of preparation during 1931, the investigation of abnormal pulse directed attention to the general subject of pulse rates. Some of the data dealing with pulse rates which were brought together in connection with this medical research are here reviewed.

The classic paper on the subject is that of Dr. William A. Guy (19) of London published in 1838. He studied the pulse rates of 100 healthy men, and observed that the rates varied when the subject was in a standing, sitting or horizontal position. He found that the pulse rate went up after taking a meal, and also that the pulse rate in the evening differed from the rate in the morning. For many years the textbooks and encyclopedias have as a result given 72 as the average pulse rate for men, and 80 for women. This results in an excess of 8 per minute for women as contrasted with only 1 per minute in the study of 30,000 lives made in connection with the *Medical Impairment Study*. The average pulse rates found among the 30,000 lives were 74.7 for men and 75.8 for women.

For years it has been known that the larger animals have a slow pulse rate and vice versa. Thus the elephant, 25–28; horse and ox, 36–50; sheep, 60–80; dog, 100–120; rabbit, 150; mouse, 700; and canary bird, 1,000.

It has been brought out by various writers that the human pulse rate varies greatly in health according to age, sex, temperament, exercise or rest, emotional states, temperature, time of day, posture, atmospheric pressure and personal idiosyncrasy.

Studies have been given showing the pulse rates of men in various occupations, and also of individuals at varying builds.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Mrs. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

ADAPTIVE COLORATION IN ANIMALS.
By Hugh B. Cott. With an Introduction by Julian S. Huxley. Oxford University Press, New York. $8.50. $9\frac{5}{8}$ x $6\frac{3}{4}$; xxxii + 508; 1940.

The force of the facts and arguments used in this work is cumulative in effect. Taken singly as isolated phenomena, they may appear to be insignificant. Taken together, and considered in relation to one another, and to kindred phenomena in other fields, they present a body of evidence which makes it appear that adaptive coloration is one of the chief attributes of the higher animals, and has been, indeed, one of the main achievements of organic evolution.

In addition to the immense body of examples presented, Cott has given the latest experimental evidence that there is a selective advantage accruing to those organisms with concealing, warning, or imitative coloration. Persons who continue to oppose the selective theory will find themselves squirming, to say the least, under the force of his arguments. The book is a scholarly treatment of protective coloration in every one of its aspects and in detail, showing how the coloration is correlated with behavior, life histories, and variable environment of the organism under consideration. The volume is greatly enhanced by numerous excellent photographs taken under natural conditions. There is a bibliography of 685 titles and a 24-page index.

EMBRYOS AND ANCESTORS.
By G. R. de Beer. The Clarendon Press, Oxford; Oxford University Press, New York. $2.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; x + 108 + 3 plates; 1940.

This volume is a revision of *Embryology and Evolution* (cf. Q.R.B., Vol. 5, p. 465). The author states in the preface that "he has seen no reason to alter the plan of my former book in the slightest degree." The revision consists of the enlargement of the various divisions of the book by the addition of new facts pertaining to the relationship existing between embryology and evolution which have been brought to light since the previous volume was issued in 1930. An important work for students of advanced biology.

STRUCTURAL ADAPTATIONS IN THRASHERS (MIMIDAE: GENUS TOXOSTOMA) *with Comments on Interspecific Relationships. University of California Publications in Zoology, Vol. 42, No. 7.*
By William L. Engels. University of California Press, Berkeley. 75 cents. $10\frac{1}{4}$ x $6\frac{5}{8}$; 60; 1940 (paper).

Two marked tendencies of behavior in this form are ground foraging and running rather than flying as a means of locomotion. Structural characters associated with the digging habit are of three sorts:

preadaptations, incidental adaptations, and primary adaptations. In association with the tendency to run there are well-marked modifications in the structure of the wing, chiefly in the proportions of the wing segments.

GENETICS

EXPERIMENTAL STUDIES ON THE NATURE OF SPECIES. *I. Effect of Varied Environments on Western North American Plants. Publication No. 520.*

By Jens Clausen, David D. Keck and William M. Hiesey. Carnegie Institution of Washington, Washington, D. C. $3.50. 10 x 6¾; vii + 452; 1940 (paper).

The difficulty of defining exactly what constitutes a species is recognized by all systematic biologists, and it is now pretty generally conceded that the only way to reach a solution of this problem is by experiment. The authors of the present work have undertaken some large scale experimentation on native California plants, the first phase of which work they now give in this report.

It is well known to everyone who has ever taken the trouble to observe it that when a plant (or an animal as well, for that matter) is widely distributed, it tends to break up into differentiated local forms. These local forms are of two distinct types, according as to whether the features that characterize them may be transmitted by heredity or not. A change in the environment may produce a change in the organism; such change is ordinarily not heritable, and is designated in the present work as a modification. But some species are divided into genetic strains (which the authors call ecotypes) which react differently to the environment. These are distinguished by heritable features, and the effect of the environment may be to preserve some at the expense of others. It must also be recalled that the features that characterize ecotypes may be somatic or cytological; they may even be physiological.

The technique employed by these investigators was first to select eleven different stations, one on the Pacific coast, one at Mono Lake, and others at various intermediate points, running up to an altitude of 3500 feet in the Sierras. At these stations were planted various wild plants, belonging chiefly to the genera *Potentilla*, *Horkelia*, *Zauschneria*, *Penstemon*, *Achillea*, and *Artemisia*. A few other genera were also represented. When plants native to one station were transplanted to other stations, they developed changes in the form of the leaves, length of stem, etc., flowered at a different date, and exhibited various degrees of polyploidy.

A tremendous amount of experimentation is covered in this report. There are also several appendices, one of which is a list of chromosome numbers that have been determined in upwards of 100 different subspecies; another is a list of 35 new names. Finally, there is a bibliography of six pages and an index of nine. The work is well supplied with photographs, diagrams, and tabulations.

PREFACE TO EUGENICS.

By Frederick Osborn. Harper and Bros., New York and London. $2.75. 8½ x 5¾; xi + 312; 1940.

With increasing education of the masses and the recent rapid advance of specific knowledge through research in the biological and social sciences has come the realization that eugenic ideals are not mere distant visions but are present possibilities. It is true, as Osborn points out, that the eugenic policy must be a conservative policy, conditioned by the slow process of social education, but "the broader aspects of eugenic proposals can now be outlined in terms acceptable both to the scientist and to the layman who is interested in other aspects of human progress. The more detailed applications of eugenics can be refined and made effective as a result of experience and with the further advance of science."

This book discusses the existing knowledge upon which our eugenic policies must be based. The author devotes considerable space to the value of population studies, an aspect of the problem the high significance of which is seldom

taken into account. The outstanding place of heredity in eugenics is adequately presented, but not to the exclusion of the environmental factors with which genetics is inextricably interwoven. Numerous positive suggestions for human betterment are given, along with the means of carrying out these suggestions through the parents, schools, doctors, clergymen, welfare workers, and those engaged in scientific research. The soundness and scientific bases of the author's arguments, coupled with his clarity of writing should make this volume equally acceptable to the biologist, the sociologist, and the lay reader.

ORGANISERS AND GENES.

By C. H. Waddington. The University Press, Cambridge and Macmillan Company, New York. $3.00. 8½ x 5⅜; x + [2] + 160; 1940.

One of the most perplexing biological problems of recent years is the nature of the causal mechanism of development. Exploration of the problem has been progressing in two fields—embryology, which studies the activity of organisers in the process of differentiation; and heredity, which deals with the genetic factors antecedent to development of adult structure. There has already been some attempt at synthesis of these fields toward a solution of this complex problem, and Waddington's book further bridges the gap. The early pages discuss experiments on organisers and their action among various vertebrates, the activation and chemical nature of the evocator, and the competence of tissues. The author next turns to the effects of genes or of substances produced by genes. After a discussion of growth and developmental patterns there is a final chapter on the theory of organization. Although the known facts about differentiation are meager, Waddington has a clear understanding of the problems involved which he transmits to his readers by frequent use of analogy to similar situations in other scientific fields. The volume contains concise definitions of all terms and an extensive bibliography.

THE BREEDING OF HERBAGE PLANTS IN SCANDINAVIA AND FINLAND. *Imperial Agricultural Bureaux Joint Publication No. 3. Imperial Bureau of Plant Breeding and Genetics, School of Agriculture, Cambridge.* 4s. 9¾ x 7½; 123 + 2 folding tables; 1940 (paper).

Since the early years of the present century the Scandinavian countries have been doing important work in the breeding of herbage plants. In this report, which is in the form of a symposium, well-known Swedish and Finnish specialists discuss the following: (Sweden) (1) Herbage plant breeding in Sweden; (2) The application of cytology to herbage plant breeding; (3) Luzerne breeding in Sweden; (Denmark) (1) Improvement of herbage plants in Denmark; (2) Some breeding experiments with timothy; (Norway) (1) Selection and inbreeding in red clover and timothy; (Finland) (1) Red clover breeding in Finland. A summary, preceding the separate articles, indicates the lessons to be drawn from the experimental work. The different crops are treated separately, most space being given to red clover and timothy. Much tabular matter and reference lists are included in the separate articles. The report concludes with lists of the stations where the plant breeding and research work is being done, their locations being indicated on two maps of the Scandinavian region.

A STUDY OF METHODS IN BARLEY BREEDING. *U. S. Department of Agriculture. Technical Bulletin No. 720.*

By H. V. Harlan, M. L. Martini and Harland Stevens. Government Printing Office, Washington. 5 cents. 9⅛ x 6; 25; 1940 (paper).

For the purpose of developing, if possible, a strain of barley superior in growth, yield, and resistance to disease, 28 parent varieties were collected from various parts of Europe, Asia, Africa, and the United States, and an enormous program of breeding and selecting was pursued over a period of 7 generations. The analysis of yields of the progeny of 379 crosses as well as that from the progeny of a composite lot resulting from the mix-

ture of the same 379 crosses form the basis for this monograph. The authors admit that the present investigation did little more than clarify the experimental procedure in their own minds, and that actually more questions were raised than were answered. They hope, however, that by further experimentation with multiple hybrid crosses they may arrive at their desired superior strain of barley.

GENERAL BIOLOGY

FUNDAMENTALS OF BIOLOGY: *Animal and Plant. Second Edition.*

By William C. Beaver. C. V. Mosby Company, St. Louis. $4.00. 8½ x 5½; 889; 1940.

Beaver has written this text with the hope that it will be of greater service to the instructor and the student by presenting the more important biological facts briefly enough to permit a complete comprehension of the subject as a whole and also to serve as a skeleton to which additional data may be added. The essentials of biology are so arranged as to permit an easier and more effective mastery of the fundamentals. In brief, the book is designed to "help the instructor help the student to help himself." This text offers the teacher many worth-while advantages. It is large enough that it can serve as a classroom text and at the same time contains much reference reading for the course in general biology. There is also the proper balance between animal and plant biology. Emphasis has been placed on the application of biology in the fields of health, wealth, and conservation. The illustrations have been carefully selected and annotated. The appendix provides definitions of biological theories and principles, important prefixes and suffixes used in biology, an extensive glossary, and a list of references for both zoology and botany. The complete index has been prepared with especial care.

INTERMEDIATE BIOLOGY.

By W. F. Wheeler. With a Foreword by Eric Lucas. Chemical Publishing Co., New York. $6.00. 8½ x 5½; xiii + 530; 1940.

It is the purpose of this book to overcome the treatment of zoology and botany as separate fields and to show the similarities between the two in every branch of biology. The first two sections describe the anatomy of representative forms of animal and plant life. Following this, the two kingdoms are compared as to cellular structure, physiology, embryology, evolution, and ecology. The aim of the book is admirable, but one cannot escape the conclusion that such a comparison in a text book works at odds against the best interests of the student. True, it tends to unify biology as a whole, but only at the sacrifice of unity within each branch of the science. It is beyond the scope of such a text to present all that should be said about both the plant and animal kingdoms even in a fundamental course. As a result, much essential material is omitted. As an example, less than nine pages are devoted to the mechanism of inheritance. The volume is adequately illustrated and contains a thorough index.

THIS LIVING WORLD: A College Course in Science.

By C. C. Clark and R. H. Hall. Drawings by Louise W. Germann. McGraw-Hill Book Co., New York and London. $3.25. 9 x 6; x + 519; 1940.

This text is the first of two volumes that constitute the bookish side of a course in general science offered, at New York University, to students whose aims are cultural and exploratory. A companion volume, *This Physical World*, will be published soon. The first volume centers its emphasis on the human body and its functioning, but the scope of the book is sufficiently extended to link animate nature to every phase of the domain in which it lives. The book opens, for instance, with a brief account of Maffei's *Scala Naturale*. It is important to bear in mind that this book is addressed to the dilettante reader. Its contents, accord-

ingly, are romaticized. There is, for example, the Norse story of Iduna, the goddess of youth. It is remarkable what a long way a very little biology can go, when emphasis is placed on making it attractive even to those who are not likely to be interested.

UNRESTING CELLS.

By R. W. Gerard. Harper and Bros., New York and London. $3.75. 8½ x 5½; xv + 439; 1940.

The intent throughout this fascinating volume has been to present the problems that biologists have recognized, the evidence they have accumulated and organized in attempting to solve these problems, the answers which they can give today, and particularly the careful manner in which they reach and check their conclusions. The book is not intended as a complete essay on biology, but is a most appealing and acceptable popularization of certain phases of the subject. The emphasis is not so much upon the facts themselves, but rather upon the various techniques employed by scientists in asceraitning these facts. Some of the rather unique chapter headings are: Odyssey of an amoeba, Architecture in miniature, Master craftsmen, Molecular traffic, Fuel for the lamp of life, Oaks from acorns, and Inherited molecules. The illustrations are a striking feature of the book. There is no bibliography, but a good index has been prepared.

HUMAN BIOLOGY

THE LIFE, LETTERS AND LABOURS OF FRANCIS GALTON. *Vol. I, Birth 1822 to Marriage 1853; Vol. II, Researches of Middle Life; Vol. IIIA, Correlation, Personal Identification and Eugenics; Vol. IIIB, Characterisation, Especially by Letters, Index.*

By Karl Pearson. The Macmillan Co., New York; University Press, Cambridge. $28.00 for the set. 11 x 7¾; Vol. I, xxiii + 246 + 66 plates; Vol. II, xi + 426 + 54 plates; Vol. IIIA and Vol. IIIB, xii + 673 + 59 plates; Vol. I, 1914; Vol. II, 1924, Vol. IIIA and Vol. IIIB, 1930.

The first of these three volumes (in two parts) appeared in 1914; the second, in 1924; and the third, in 1930. In view of the fact that the complete set is now available at a price considerably below the original, we bring this important work—a monument not only to the subject but also to the author—to the reader's attention.

In the introduction to the final volume Pearson explains that he is not a man of letters (a statement which will not meet with universal agreement) and that in writing this book he was actuated solely by the desire to tell the story of his teacher and friend. One reason for taking this position was that he was too close to Galton to do for him what, for instance, West did for Darwin, or Stejneger for Steller. The size of the work bears witness to Pearson's desire to make it as exhaustive as possible. It is a source book for the scholar rather than a narrative for the general reader. The Galton family preserved a wealth of material in the shape of correspondence, both by and to Sir Francis, and this they generously submitted to Pearson with permission to publish, a permission of which he availed himself extensively, for he realized the importance of preserving these documents for posterity.

The greater part of the first volume is occupied by a discussion of Galton's ancestry—a thoroughly consistent way to begin the life story of the founder of the science of eugenics. Everyone knows that Francis Galton was the grandson of Erasmus Darwin and the cousin of Charles Darwin, but it is not so well known that he was also the descendant of Robert Barclay, the apologist, or that he was related to Elizabeth Fry, who frequently visited the Galton family, and exercised a great influence on Francis in his early youth.

Francis Galton, ninth child of Samuel Tertius Galton and Violetta (Darwin) Galton, was born February 16th, 1822, at The Larches, near Sparbrook, Birmingham, in a house built by Withering, the botanist, and occupied subsequently by Priestley, the chemist. His early educa-

tion was in charge of his eleven-year-old sister, Adèle, also a gifted child. At the age of two he could read, sign his name at three, recite most of Scott's "Marmion" at five, and read the *Iliad* in the original at six.

Lack of space precludes discussion of Galton's formal education at Boulogne, Birmingham, and Cambridge, where he studied medicine to please his father, mathematics to please himself, and classic languages to please his teachers; or of his trip to Giessen, ostensibly to study organic chemistry under Liebig, but which he converted into a pleasure jaunt to the Bosphorus and the Mediterranean; or of his later expedition to Africa to settle the differences that had arisen between the natives and the Boer migrants who had settled in British territory. Although all these phases of Galton's activity are adequately considered by Pearson mere mention of them must suffice here, for his scientific career did not really begin until after his marriage to Louisa Butler on August 1st, 1853, when he settled down to devote himself to the scientific pursuits that were destined to occupy his attention during the remainder of his long life.

In the next few years he investigated such diverse matters as solar eclipses, heliography, relief maps to be made in pairs and viewed through a stereopticon, meteorological maps, anticyclones (which he discovered and named), the possibility of constructing a pantagraph that would reduce different scales simultaneously in different ratios, the practical utilization of wave power, mensuration of the altitude of clouds, and even spiritualism. His old interest, mathematics, was not neglected, but it was now made to serve the biological sciences, chiefly anthropology. The outcome of the mathematical study of human inheritance was the epoch-making *Hereditary Genius* which won the praise of Darwin and made inevitable the science of eugenics.

The rest of Galton's activities, such as his study of finger prints, composite photography of criminals to see if a "criminal type" could be isolated, the establishment of the Eugenics Laboratory at the University of London, as well as the more personal side of his biography, the passing of his contemporary friends and the relatives of his own generation, have received the treatment they merit. In his later years he received the medal of the Linnean Society and was knighted by King Edward VII.

Galton's method, according to Pearson, was to formulate his problem, and then to devise his experiments to throw light on it. In this respect he was the antithesis of Darwin, who first accumulated his observations and then let his imagination play on them. The difference between Newton and Bacon is roughly parallel—it is perhaps that between the mathematical and biological temperaments.

Pearson did an admirable work in preserving for future generations a living portrait of one of the most significant and versatile figures of the nineteenth century—the associate of Darwin, Spencer, Huxley, Wallace, Tyndall, and Carpenter, all of whom he outlived. The book has an excellent index of forty pages, and its wealth of plates adds greatly to its value.

WHY FRANCE LOST THE WAR: *A Biologic and Economic Survey*. *By A. Reithinger. Veritas Press, New York.* $1.25. 7¾ x 5¼; 75; 1940.

The unexpected collapse of the French nation in a war largely of its own choosing has engendered a lot of more or less nonsensical speculation as to just what happened. The present work forms a pleasing contrast to most of these, for it contains nothing of a speculative nature. In it the author merely discusses the demography of France, and shows that the present condition is the inevitable result of the first world war.

Under Louis XIV one out of every three Europeans was a citizen of France. At the turn of the century this proportion was somewhat reduced, but France was still the most populous European state. Today only one European out of twelve is a French citizen, and Russia, Germany, Great Britain, and Italy can boast larger populations.

But this is not the only difference in demography between France and Ger-

many. There is also a difference in distribution among age groups. The nonproductive groups, both children and aged, constitute a larger proportion of the total population in France than in Germany. And finally there is a difference in the replacment rate. While in both countries the population is increasing, the increase in France is dependent upon immigration. Except for immigration the French population could not maintain itself.

All this has a direct bearing upon the size of the army that either nation could maintain in the field. If both nations maintained equal armies, the French army would represent about three times the proportion of the productive population as would the German army. And if both armies suffered equally the German population could supply replacements much more rapidly than could the French. This would hold good not only for the armies actually in the field, but for that part of the population that remains at home engaging in gainful occupations to supply wealth, foodstuffs, and other needed commodities to support the army.

The author is a German and his viewpoint is that of the modern rulers of Germany. He puts the entire responsibility for the present war on Great Britain, saying that Russia, Germany, Italy, and France could have adjusted their vital interests sensibly and without difficulty had not Great Britain undertaken a coalition war against Europe, the tragedy of which was the alignment of France with the wrong side. This thought, reiterated more than once in the book, is likely to influence the thoughtful reader to question the author's intellectual competence to pass judgment on the collapse of France, but if the thoughtful reader can forget the prejudices, and overlook the ill-disguised attempts at propaganda of the author, he will be likely to conclude that so far as the purely factual conclusions are concerned, the author has convincingly proved his thesis.

HUGH YOUNG, *A Surgeon's Autobiography. By Hugh H. Young. Drawings by William P. Didusch.* *Harcourt, Brace and Co., New York.* $5.00. 9¼ x 6; xii + 554 + 6 plates; 1940.

Students of American history will be interested in the first 88 pages of this book. Hugh Young's grandfather fought for the republic of Texas, was a colonel in the Mexican War and a general in the Civil War, and his son, father of the surgeon, commissioned brigadier general at the age of twenty-six, was severely wounded and captured in the defense of Atlanta. After the war the two generals returned to San Antonio, Texas. San Antonio was at that time one of the liveliest and wooliest of the Western towns and here the autobiographer was born and spent his boyhood. By the time he entered the University of Virginia, where he earned his B.A., M.A., and M.D. within the space of four years, he had already had experience as a newsboy, horse trainer, athlete, carpenter and mechanic (his proficiency in these latter trades stood him in good stead later in devising new instruments, and provided him also with his first lesson in the deceits of business—he never did collect the $6.50 a friend promised him for a buggy body he made at the age of fifteen), printer, editor, and cartographer. With the $40 fee for his first operation in San Antonio he bought a ticket for Baltimore, where he continued his studies in surgery, served for a few months as bacteriologist at a children's sanitarium, assisted Dr. Halsted on the surgical staff of the Johns Hopkins Hospital and (two years later) was put in charge of the department of genito-urinary surgery, the specialty to which he has devoted a long and distinguished service and in which he won international fame.

About a quarter of the book is devoted to technical details of urological cases and various operations which illustrate his new techniques and instruments. This part, which is likely to be skipped by the squeamish, is illustrated with 103 excellent anatomical and surgical drawings by William Didusch.

Of more general interest are the descriptions of his patients (Diamond Jim Brady, who out of gratitude made possible the Brady Urological Institute, heads the

list), and friends, his service overseas with the A.E.F., his work on the venereal disease problem in the British, French and United States armies, his extensive travels, and his active contributions to the cultural life of Baltimore.

Although this most interesting and frank autobiography interspersed with amusing anecdotes will be of primary interest to medical men, it is bound to find a wider circle of readers.

EDUCATION AND THE BIRTH-RATE. *A Social Dilemma.*

By Grace G. Leybourne and Kenneth White. With a Foreword by A. M. Carr-Saunders and an Introduction by R. H. Tawney. Jonathan Cape, London. 8s. 6d. net. 8 x 5¼; vii + 375 + 9 folding tables; 1940.

During the last fifty years in most countries there has been a remarkable increase in the number of persons who receive a secondary school education or professional training. This phenomenon, as all realize, tends to raise the cultural level of the population. However, and in addition, other problems are created and these are especially evident in times of economic or political upheaval. The authors for the first time examine these problems with respect to England. In their opinion the desire for a higher education results mainly from the combined manifestations of snobbery and of the hope for an improved socio-economic status. The first general consequence is that parents attempt to limit the number of offspring. The second and most important consequence is that technically untrained but cultured (?) youths are dumped on a labor market which has no demand for such a commodity. Other consequences are (a) the need for more schools, and (b), specifically for England, the threat to the existence of private schools from the increased competition of government institutions. These inferences have been drawn from an analysis of available statistics on school population and cost of education. Obviously they cannot be considered as definitively proven. For example, there is no factual evidence that the desire to give the offspring a higher education is causally related to contraceptive efforts. However, the general economic and labor problems that emerge from the increase in the size of secondary school population are of the highest importance and the authors have done well to bring them out into the open. It is hoped that the future will see a more intensive analysis of the question in this country.

A DOCTOR'S HOLIDAY IN IRAN.

By Rosalie S. Morton. Foreword by Hugh S. Cumming. Funk and Wagnalls, New York and London. $3.00. 8¼ x 5½; xi + 335; 1940.

The awakening of the Near East, at a time when Europe is deeply engaged in mutilating her own more recent civilization, may possibly mean a surging upward of forces that have long been lying fallow. A great people and a great culture once flourished here. Would it not be possible for something of that greatness to return? Dr. Morton does not say this in so many words, but she is deeply impressed with the new social and national consciousness that is emerging in what is now Iran—and was so-called before the land was given the name of Persia. Through sheer ability the present Shah, Riza Pahlavi, has risen, like our own Lincoln, from poverty to become a great leader. As in Turkey, great changes are taking place in the lives of the people, but the task is far more difficult than in Turkey because of the greater isolation of Iran and its more burdensome traditions and customs. Nevertheless, the Shah is rebuilding a nation and giving his people self-confidence and self-control.

Dr. Morton, a keen observer, writes most interestingly of her travels throughout the land. Comparing the usefulness of the camel and the automobile, she is inclined to agree with one who told her

As an all-round investment nothing equals a camel, for when it is born it costs nothing, it finds its own food, wears far longer than an automobile, needs no repairs, can carry passengers and freight, is always reliable, goes well on rough roads and in the sand, needs no garage, gas or oil; receives water at conven-

ient times and places and uses it economically; when too old to work it becomes a religious sacrifice, after which its meat can be eaten. Its skin is useful for many purposes, its bones are used for jewelry, both carved and painted. In comparison, what value is an automobile? It cannot even reproduce itself!

JAMES GEORGE FRAZER: *The Portrait of a Scholar.*

By R. Angus Downie. Watts and Co., London. 5s. net. 8½ x 5½; ix + 141; 1940.

To those who have followed Sir James Frazer's writings this little volume, written by one who has been closely associated with the great anthropologist in recent years, will be of much interest. Downie herein gives a brief biographical sketch of Frazer, a survey of his work in the classical and anthropological fields, as well as his literary studies of modern times and writers, and concludes with an estimation of the influence which Frazer has had on human thought.

In 1888 Frazer wrote "Taboo" and "Totemism" for the *Encyclopaedia Britannica.* It was in the preparation of these articles that he began "a systematic application to anthropology and especially to a study of the backward races of men whom we call savages and barbarians." The *Golden Bough* originally appeared in two volumes in 1890. Later (1911–1915) it was revised and extended to the well-known 12 volumes, with a 13th volume *Aftermath* appearing in 1936. In 1898 appeared the 6 volume edition of Pausanias —Frazer's greatest work as a classical scholar. *Totemism and Exogomy* appeared in 1910, and *Folk-Lore in the Old Testament: Studies in Comparative Religion, Legend, and Law* in 1918. Frazer's many works have been written by his own hand "with no assistance beyond that of a steel pen."

The volume concludes with a short bibliography of Frazer's works (56 titles) and an index.

I REMEMBER. *The Autobiography of Abraham Flexner.*

By Abraham Flexner. Simon and Schuster, New York. $3.75. 9¼ x 6⅛; xii + 414 + 17 plates; 1940.

It now seems difficult to realize that practically all the advances made in medical education in the United States have occurred since 1908, the year in which Abraham Flexner began, for the Carnegie Foundation for the Advancement of Teaching, his study of the status of and conditions in the medical schools in this country and in Europe. His report was the impetus to the development of the full-time medical school with raised entrance requirements and complete clinical and laboratory facilities—one school reported on in 1908 had a "physiological laboratory" consisting solely of one sphygmograph.

In this well-written autobiography Flexner gives a detailed, but not tiresome, account of his investigations into the state of medical schools, as well as of the methods, advanced for their time, employed in his fifteen-year career as teacher in his private school in Louisville, Kentucky, which preceded his connection with the Carnegie Foundation. Likewise he relates in a modest manner the part he played in the investigations conducted for the Carnegie and Rockefeller Foundations into the teaching in universities, colleges, high schools, and elementary schools in the United States, and the progress and changes of educational methods which followed throughout the country. The history of the Institute for Advanced Study which he founded at Princeton in 1930 and still directs, is also told.

This book is well indexed, and the whole constitutes an interesting and adequate history of modern education in the United States.

THE PREHISTORIC FOUNDATIONS OF EUROPE. *To the Mycenean Age.*

By C. F. C. Hawkes. Methuen and Co., London. 21s. net. 8½ x 5½; xv + 414 + 12 plates + 6 maps and tables; 1940.

This book will delight the thoughtful reader. It presents a great mass of data on early European culture but in such an

orderly fashion that the reader soon realizes that here at last is unity between historic and pre-historic times. In other words, civilization of today can now, in many of its phases, be traced far, far back into pre-historic times. The author finds the whole story of human culture in Europe

> indissolubly bound up with the physical character of Europe itself, the landscape and climate which have made the environment of human culture. And that character is unique.
>
> The diversity of altitude and structure between its Alpine spine and the Mediterranean and Baltic depressions to the south and north give life and landscape a wide range of conditions for its relatively small size. In short, it is capable, under secular changes of landform and climate, of forming a natural paradise for the play of *adaptive vitality* [our italics].

It is in adaptive vitality that the author finds grounded the moving pattern of European achievement. "The movement of the pattern, the instability of the balance, seem throughout characteristic of historic process in Europe, as against the 'changeless East', which invented civilization only to stagnate in it. . . ." "The instability of Europe's equilibrium has been not its weakness but its strength; . . ."

Twelve plates, 6 maps, tables, and many text figures add to the interest of the volume. Twelve pages of bibliographical notes follow the text and there is an excellent working index.

THE ANTHROPOLOGICAL JOURNAL OF THE INSTITUTE OF HISTORY AND PHILOLOGY. *Academia Sinica. Volume 1, Part 1.*

Edited by T. L. Woo, S. S. Ling and S. Y. Liang. The Commercial Press, Shanghai. 10½ x 7½; 233; 1938.

T. L. Woo contributes the first paper "An anthropometric study of the Chinese clavicle based on the Hsiao T'un and Hsiu Chiu Shan specimens" in this interesting new publication. Two series of clavicles (155 male and 95 female) from different regions of China and representing populations of the Sui-T'ang dynasties and of modern times were studied with respect to ten indices, for some of which new methods of measurement are developed. The right clavicles were found to be, on the average, shorter, stouter, and more curved than the left. A marked differentiation between the clavicles of the two sexes was revealed in average absolute measures but in the measurements of shape no marked sexual differences were found. This paper is in English. The following three articles, A study of the U-Man and Pei-Man of Yunnan in the T'ang dynasty, by S. S. Ling; On the divination of the Moso tribe in Yunnan, by Y. K. T'ao; and Miao stories of a great flood and the legends of Fu-hsi and Nü-wa, by Y. F. Ruey are in Chinese. The fifth study, in English, on the glabella prominence of the human cranium, is based on series of skulls from various parts of the world preserved in the Museum of the Institute of History and Philology (Chinese specimens) and the Museum of the Royal College of Surgeons, London. The racial types of cranium differed quite appreciably in both size and shape of glabella. New instruments for the measurement of hand and foot circumferences are described and illustrated in the closing article (in German) by T. L. Woo.

FAMILY INCOME AND EXPENDITURES. *Middle Atlantic and North Central Region and New England Region. Part 1, Family Income. U. S. Department of Agriculture. Miscellaneous Publication No. 370. Consumer Purchases Study. Urban and Village Series.*

By Day Monroe, Elizabeth Phelps and Idella G. Swisher. Government Printing Office, Washington. 50 cents. 9⅛ x 5¾; iv + 446; 1940 (paper).

FAMILY INCOME AND EXPENDITURES: *Pacific Region and Plains and Mountain Region. Part 1, Family Income. U. S. Department of Agriculture. Miscellaneous Publication No. 356. Consumer Purchases Study. Farm Series.*

By Day Monroe, Dorothy S. Martin, Margaret Perry, and Kathryn Cronister. Government Printing Office, Washington. 30 cents. 9⅛ x 5¾; iv + 276; 1939 (paper).

These two books refer specifically to the subject of family income. The first covers investigations in small cities and villages of the Middle Atlantic, North Central and New England Regions. The second contains the results of investigation of income of farm families of the Pacific Region, and Plains and Mountain Regions. The information obtained is presented mainly in statistical form, and is broken down into studies of family income from many different angles, dealing with income and earning capacity as related to age, sex, occupation, place of residence, home ownership, etc. These studies will be of use to economists, and to biologists investigating the subject of population.

POPULATION POLICIES AND MOVEMENTS IN EUROPE.

By D. V. Glass. Oxford University Press, New York; The Clarendon Press, Oxford. $6.00. 8½ x 5½; vi + 490; 1940.

In this volume Glass presents a comprehensive review of a major proportion of the recent literature on population problems in an effort to clarify the two main questions with which students are concerned, to wit: the future trend of natality and the general effects of such a trend. The author reexamines first the decline in the natality of England and the factors which may have influenced it. The inferences that he draws follow the accepted pattern. He then proceeds to a survey of state intervention and compares the demographic policies of the Scandinavian countries, France, Belgium, Germany, and Italy. This survey comprises the greatest portion of the book and constitutes its most remarkable feature since the author has brought together the available information on the matter and discusses fully the underlying theories and practical applications of the governmental policies. The final conclusions reached by the author are, in essence, (a) that there is not sufficient knowledge to permit accurate predictions of the future trend in natality; (b) that the attempts to increase natality by state intervention have not been successful. With these conclusions everyone will agree. The author also describes briefly in an appendix the methods of determining certain statistical constants useful for demographic studies. An ample bibliography is included and although omissions are to be noted this volume will be found useful as a source of reference to the latest reports on the subject.

PASCUA: *A Yaqui Village in Arizona.*

By Edward H. Spicer. University of Chicago Press, Chicago. $3.50. 8⅛ x 5¼; xxxi + 319 + 13 plates + 9 charts; 1940.

The village of Pascua is geographically part of the city of Tucson—culturally it is completely isolated from the dwellers about it. Linguistically the Yaqui Indians are not very closely related to those of Arizona, and they have had a widely different history. Whereas the native Arizona Indians are mostly pueblo dwellers and do not take kindly to Nordic civilization, the Yaquis adopted Spanish culture, and entered the United States only about half a century ago, as the result of persecution at the hands of the Diaz administration. They are not Mexicans, they are not "Saxon Americans," they are Indians, but of a widely different type from those about them.

Such a group of people (there are only about 2500 of them altogether, and less than 500 in Pascua) cannot but be of great interest to the student of sociology, and the author of this book has produced an excellent piece of work, covering all phases of the life of these people—their work, their play, their language, their religion, their education, what food they eat, how they conduct their ritual dances, their baptisms, and their funerals. There is a bibiliography of two pages, an index of seven, and numerous photographs. It is an excellent study of an isolated cultural community, quite unlike anything else in the United States.

ESSAYS IN POLYNESIAN ETHNOLOGY.

By Robert W. Williamson. Edited by Ralph Piddington, with an analysis of

recent studies in Polynesian history by the Editor. The Macmillan Co., New York; The University Press, Cambridge. $7.00. 9½ x 6½; xlii + 373 + 10 plates + 2 maps; 1939.

This handsome volume completes the final series of studies that Williamson had undertaken in the social anthropology of Central Polynesia. Three volumes have previously appeared on the following general topics: social and political systems; religious and cosmic beliefs; and religion and social organization. Much material, however, still remained to be recorded and the first five chapters of the present work embody these posthumous manuscripts. The chapter headings are as follows: Warfare in Central Polynesia; Kava in Samoa and Tonga; Kava in other Polynesian islands; The *Arioi*; Sex, courtship and infanticide. The main part of the volume (Part II) is a discussion of historical problems in Polynesia. It

represents an attempt to assess the present position of historical studies in Polynesia, with special reference to certain views which have been propounded since Williamson's death. It is concerned with certain recent theories of Polynesian origins and these have a bearing on the controversy between the historical and functional schools, a controversy which has reached a critical stage during the present decade, and which is unquestionably the most important issue in anthropology today.

Piddington contributes the preface to this scholarly work; a bibliography with abbreviations precedes the textual matter; a map, a number of plates and numerous footnotes occur in the text; and an appendix on Assam parallels and a detailed index conclude the volume.

BERKELEY MOYNIHAN, *Surgeon.*

By Donald Bateman. With a Preface by Lord Moynihan. The Macmillan Co., New York. $4.00. 8½ x 5½; xv + 354 + 8 plates; 1940.

This interesting volume is an account of the life of the first Baron Moynihan of Leeds, one of the dominant figures in British surgery during the first three decades of this century. A man of high ideals, striking force of character, and exceptional charm of manner and mind, Moynihan had the great honor of being the first provincial surgeon to be made President of the Royal College of Surgeons—an unwritten rule being that the President should be a member of the staff of a London teaching hospital. He remained, however, always loyal to Leeds and continued to do much of his practicing there.

Many physicians and surgeons in this country will remember Moynihan's speeches (he was a famous orator) when in the fall of 1917 he was assigned by the British Government to advise the U. S. Army Medical Corps in its preparations. His speeches fell into two classes: those of a strictly surgical interest, and those upon the general aspects of the war. These latter, before lay audiences for the purpose of advancing the allied cause, he delivered with great effect.

Bateman has included in the volume many interesting letters and a number of illustrations.

TODAY AND DESTINY: *Vital Excerpts from The Decline of the West of Oswald Spengler. Arranged with an Introduction and Commentary by Edwin F. Dakin. Based on the text of the Authorized Translation of Charles F. Atkinson. Alfred A. Knopf, New York.* $2.75. 8½ x 5⅝; viii + 364; 1940.

Spengler belonged to that very small group of social philosophers that does not subscribe to the prevalent view that every day in every way our civilization is proceeding to a higher and more sublime level. Moreover, he believed, as many others have, that political history proceeds in inevitable cycles. Twenty years ago he was so bold as to predict that the 19th century liberal form of government would gradually be displaced by dictatorship and all that this entails. These views never pleased the professional and orthodox sociologists who therefore made a point of demonstrating the technical and methodologic inadequacies of Spengler's work. Dakin, who apparently has read Spengler carefully, protests against the injustice done him and particularly against the label of Nazi which has been attached to him. To indicate the objec-

tives of Spengler's study Dakin has brought together in this volume excerpts from the famous *The Decline of the West*. It is the editor's intention to show that Spengler deplored the end of democracy and was really attempting to awaken the people to the need for defending it. Dakin seeks to do the same now by reiterating Spengler's words.

ORDER AND POSSIBILITY IN SOCIAL LIFE. *By Douglas G. Haring and Mary E. Johnson. Richard R. Smith, New York.* $4.00. 9¼ x 6; xii + 772; 1940.

This textbook on sociology takes its title from an essay by the late F. H. Giddings to whom the book is dedicated. The subject matter is arranged under five main headings. In the first part, certain aspects of the social behavior of several peoples not of western civilization are described. The second and third parts deal summarily with evolution, heredity, and the physiological and psychological manifestations of man. Books four and five touch on the formation of societies and their varieties. This work has two distinctive characteristics. The first is that the authors have had recourse to extensive citations from the authorities in the fields covered. The second is that an attempt has been made to introduce into the picture of human social behavior its biological foundations. The attempt has not been completely successful because a net separation between social action and biological attributes is still noticeable. However, the authors have definitely progressed further along the objectives of human biology than most sociologists have and this volume is a good presentation of the essentials of the subject. It is to be deplored that the book lacks a list of references or some selected bibliography.

CULTURE AND SURVIVAL. *By Guy Chapman. Jonathan Cape, London.* 8s. 6d. net. 8 x 5¼; 243; 1940.

The general purpose of this book is "to discuss a single aspect of the growth and decline of population in England and Wales, and more particularly the problem of the decline which today threatens. The aspect studied is the connection between what may be called culture, and the family." In the course of a survey of the changes in the nature of consumption goods over some centuries, the author was impressed with the rise in the standard of living and the decline in fertility; with the breaking up of rural culture and the development of leisure activities in the large cities. He shows how labor and capital have become more and more engaged in the production of goods for minor comforts such as labor-saving devices and amusements for the leisure hours of those who, only recently having acquired leisure, have no stabilizing background for the use of such freedom. This makes for a dangerous situation when the country must be put to the task of producing goods for war purposes. In the final chapter, "The Dilemma," he compares the industrial society and the changes in the birth rate of Germany with those of England.

Bibliographic footnotes are given throughout the text but the volume is not indexed.

YOUR LIFE IS THEIR TOY. *Rackets—Social Service and Medical.* *By Emanuel M. Josephson. Chedney Press, New York.* $3.50. 9⅛ x 6⅛; [12] + 449; 1940.

Josephson divides his "expose" into two parts—the social service rackets and the medical rackets. The author uses the word "racket" in the colloquial sense, in all of its shades of connotation, and as he proceeds there are few agencies and philanthropies which escape castigation. Josephson believes that every age has had its charlatans, quacks and medicine men and that the present period has produced a bumper crop which is exploiting mankind unmercifully. He further thinks that the consequence of this welter of exploitation is the sacrifice of human comfort, happiness, health, and life. The author states that his motive in presenting this treatise is not malice, but

rather the desire "to protect the health and life of the public and to see justice done." The book is well documented and presents a most challenging and provocative discussion of various aspects of modern society. A good index is appended to the volume.

THE NEW NOAH'S ARK.

By André Demaison. Translated by Eric Sutton. The Macmillan Co., New York. $2.50. 8 x 5⅜; [10] + 294; 1940.

Sailing along the coast of Africa in an old schooner Demaison made a temporary business of collecting wild animals. The pages of his book are filled with the color of the sea and jungle. He describes the superstitions and prejudices of his half-caste crew and of the native tribes that he met along the way. But particularly interesting are the accounts of the behavior of the beasts. Yet the book is purely an adventure story and sheds no new light on the enigma as to how much of the intelligence of animals is due to actual thought and how much to instinct or response to physiological processes. The ordinary course of the voyage was punctuated by such unexpected events as the breaking loose of the water buffalo, the accidental arrival in a port rife with yellow fever, and as a climax, the poisoning of the crew, shipwreck, and escape with the precious cargo.

ESSAYS IN HISTORICAL ANTHROPOLOGY OF NORTH AMERICA. *Published in Honor of John R. Swanton in Celebration of His Fortieth Year with the Smithsonian Institution. Smithsonian Miscellaneous Collections, Volume 100 (Whole Volume).*

The Smithsonian Institution, Washington, D. C. $2.00. 9⅛ x 6; [6] + 600 + 16 plates + 1 folding map; 1940 (paper).

Thirteen anthropologists, most of whom are ethnologists, have contributed to this volume which honors John R. Swanton. Kroeber introduces the work with an evaluation of Swanton's contribution to the knowledge and methodology of ethnological investigations. The articles, as the title indicates, are concerned with North American primitive peoples. The continent is effectively if not completely covered since the papers deal, respectively, with the peoples of Virginia and the Southeast, the Northeast, and of the northern Mississippi Valley, of the central and northern great plains, the Southwest, the great basin area, and the subarctic regions. It may be said that each paper represents a scholarly piece of work and this volume constitutes a survey of existing information on the subject.

IN SEARCH OF COMPLICATIONS. *An Autobiography.*

By Eugene de Savitsch. Foreword by Arthur Krock. Simon and Schuster, New York. $3.00. 9¼ x 6; [6] + 396; 1940.

de Savitsch is still a young man, being about 37 years old, but he has already gone through more experiences than are usually acquired in a whole life time. Born in the higher Russian bourgeoisie, he fled at 14 from the Bolsheviks. He became for a short time an officer in the ill-fated White Russian army and then via Japan he migrated to this country. Here he was a floor-walker in a department store, nearly starved himself and contracted tuberculosis. When he recovered he turned to medicine for some reason and finally obtained a medical degree from Chicago University after a stopover at Colorado. These and other adventures are told with the style of a seasoned raconteur and patently aimed at bringing out the humor of the situations. As a result the book is interesting and amusing except in those very rare occasions when the author becomes pontifical about scientific matters.

CHINA'S ANIMAL FRONTIER.

By Clifford H. Pope. The Viking Press, New York. $2.50. 9 x 6; 192; 1940.

Everybody knows about Roy Chapman Andrews and some of the things he obtained from the desert of Central Asia.

But the story of the expedition itself is not so well known. In this work Clifford Pope, a member of the party, tells this story, and brings to life before the readers' eyes the various members of the expeditionary force, mostly Chinese, who officiated as artists, preparators, interpreters, cooks, etc.

The book is well illustrated and makes delightful reading. It should appeal to everybody. Although it contains much scientific matter, such as the discussion of alligators and dolphins, it is not primarily a scientific treatise but an adventure story.

RURAL POPULATION DENSITY IN THE SOUTHERN APPALACHIANS. *United States Department of Agriculture: Miscellaneous Publication No. 367.*

By Francis J. Marschner. United States Department of Agriculture, Washington, D. C. 25 cents. 9⅛ x 5¾; 18 + 1 folding map in pocket; 1940 (paper).

In general the population density of the Southern Appalachian region conforms to the rule that man will congregate in locations where the land is most productive. However, the present report shows that this relationship is not consistent, being complicated by numerous social and economic factors, and others the nature of which are less readily understood. No attempt is made to interpret these interrelationships, so the report is largely a factual presentation. It is accompanied by an excellent large population map of the area.

ZOÖLOGY

PRINCIPLES OF ANIMAL BIOLOGY.

By Lancelot Hogben. Illustrations by J. F. Horrabin. W. W. Norton and Co., New York. $3.75. 8 x 5⅜; 415; 1940.

In this work Hogben has sought to achieve two ends—to write a practical textbook of elementary biology for use in schools and colleges, and to write a popular treatise that would hold the interest of the reader who has but little technical knowledge, and from which he might derive both pleasure and profit. Since these ends obviously could not be achieved simultaneously the book is divided into two parts. The first of these is built up around descriptions and discussions of the physiology and reproduction of the frog, though the other animals are by no means neglected. Each chapter is supplemented by a list of questions and subjects for home study.

The second part gives the author an opportunity to demonstrate his mastery of the art of self expression in writing. Beginning with the well-known passage from Gerard's *Herball* of 1594 dealing with the development of the barnacle goose (the original illustration from the *Herball* is reproduced) the author goes on to a discussion of the taxonomic systems of Linné, Cuvier, Lamarck, Leuckart, and Milne-Edwards, explaining not only how these systems differ, but also why.

The critical reader is apt to feel that this historical approach might have been reinforced by the inclusion of Aristotle, but the fact is that the author seems not very kindly disposed toward that worthy. To Aristotle he attributes responsibility for the parasitic growth of superstition that clouded the proto-science of the dark ages and impeded its progress, just as the progress of a ship is impeded by the barnacles on its bottom.

The work is supplemented by four appendices, a ten page index, and numerous excellent illustrations.

AN INTRODUCTION TO ENTOMOLOGY. *Ninth Edition Revised.*

By John Henry Comstock. Comstock Publishing Co., Ithaca, N. Y. $5.00. 9 x 6; xix + 1064; 1940.

To students of entomology the name of Comstock has been both familiar and authoritative since 1888 when the author published his first *Introduction to Entomology*. During the early '90's this first work was replaced by *A Manual for the Study of Insects*, which in turn was later (1924) replaced by the first edition of the present text under the title of *An Introduction to Entomology*. The present edition (9th) under the editorship of

Glenn W. Herrick maintains the same high standard of workmanship responsible for the popularity of the earlier editions.

The material of the text is presented under two heads; (1) The structure and metamorphosis of insects; and (2) The classification and life histories of insects. In the former section are discussed such pertinent topics as the characteristics of insects and their nearest relatives, the external and internal anatomy of insects, and the factors relative to the metamorphosis of insects. The later section presents not only the keys for the classification of the 26 orders of insects, but also a short description of the adult, eggs, and immature stages of each insect, a discussion of the sex and seasonal variation in the color, form and activity of each form, as well as the distribution, feeding and breeding habits of the various species.

The text is amply supplied with illustrative material and is concluded with a bibliography of 521 titles and a detailed index.

INSECT TRANSMISSION OF PLANT DISEASES. *By Julian G. Leach. McGraw-Hill Book Co., New York and London.* $6.00. 9 x 6; xviii + 615; 1940.

One has only to glance at the appendix of this pioneering textbook to realize how many insects have been found to be vectors of plant pathogens. That tobacco mosaic is a plant disease nearly everyone must know, but that it is insect transmitted is uncommon knowledge. Proof of the rôle that insects play in the spread and development of plant diseases has accumulated rapidly since Waite, in 1891, made it evident that the bacillus of blight is carried from flower to flower by the honeybee. The author of the present volume became absorbed in this new field of study in 1923, when an investigation of the blackleg disease of potatoes led him to study the rôle of flies in the transmission of bacterial soft rot generally. Leach's work, first at the University of Minnesota and now at the University of West Virginia, led the late Dr. Royal N. Chapman to suggest that he write a book to signalize the emergence of this new field of research. Here is the book. It is not intended to be an exhaustive treatise, yet it runs to 17 well-organized chapters, each with its list of references. The text is copiously illustrated. Like other works which are the first in their fields, this book promises to become a classic.

A GENERAL ZOOLOGY OF THE INVERTEBRATES. *By G. S. Carter. Foreword by Julian S. Huxley. The Macmillan Co., New York.* $5.50. 8½ x 5⅜; xxviii + 509; 1940.

As a rule books on invertebrate zoology go into the details of morphology and present only a brief discussion of the general biological problems involved in the process of living in a particular environment. It is the purpose of this book to discuss more completely these biological principles. Hence it is not a text-book of descriptive zoology, but is designed as supplementary reading to broaden the scope of the student, particularly in invertebrate physiology and ecology.

There are four sections in the volume, the first of which describes the fundamental physical, chemical, and biological properties of living cells. The second section discusses the problems of differentiation, growth, regeneration, form, and organization in the multicellular body. Next treated is the comparative physiology of the metabolic processes and of sensory response. The final section is devoted to the general problems of the life cycle, behavior, habits, and evolution of invertebrates. Some biologists may differ with the author in the details of certain opinions which he expresses, but on the whole the book is well written and logically presented. It contains a good index and a bibliography by topics.

THE BIRDS OF BUCKEYE LAKE, OHIO. *Miscellaneous Publications, Museum of Zoology, University of Michigan, No. 44.*

By Milton B. Trautman. University of

Michigan Press, Ann Arbor. $2.50. 10 x 6½; 466; 1940 (paper).

This survey sets a fine standard, and shows what can be done in an intensive study of the avifauna in a small area. The main part of the work was done by the author, but many others contributed to the study which was made over a twelve-year period. An attempt is made to give as complete a conception of the status of each bird species in the Buckeye Lake area (in the vicinity of Columbus) throughout historic times as is possible with the data at hand. This includes a brief account of the interglacial and postglacial history of the Buckeye Lake area, from Wisconsin time to the beginning of historic time; a brief discussion of the invasion and occupation of the area by the white man, and his influence and effect upon the topography, flora, and fauna, especially as regards birds; and a compilation of the literature concerning the bird fauna, and of unpublished data on it and on conditions in the area before February 1, 1922. Thereafter, the results of twelve years of bird observations made between February 1, 1922, and February 1, 1934, are given. Citations of literature, photographs and indexes of common and scientific names of plants and animals conclude the volume.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society. Volume XXV, Part 2, Numbers 11–18.*

New York Zoological Society, Zoological Park, New York. $2.00. 10½ x 7; 163; 1940 (paper).

The following papers are contained in this number: Plankton of the Bermuda Oceanographic Expeditions. IX. The Bathypelagic Caridean Crustacea, by Fenner A. Chace, Jr. (64 text-figures); Eastern Pacific Expeditions of the New York Zoological Society. XIX. Actiniaria from the Gulf of California, by Oskar Carlgren (8 text-figures); Morphological and embryological studies on two species of marine catfish, *Bagre marinus* and *Galeichthys felis*, by Daniel Merriman (5 plates; 9 text-figures); Propagation of the electric impulse along the organs of the electric eel, *Electrophorus electricus* (Linnaeus), by C. W. Coates, R. T. Cox, W. A. Rosenblith and M. Vertner Brown (1 plate; 3 text-figures); Notes on the display forms of Wahne's Six-plumed Bird of Paradise, by Lee S. Crandall (3 text-figures); Acute hemorrhagic gastro-enteritis in a giant panda, by Leonard J. Goss; Two new species of trematodes from the deep sea scorpion fish, *Scorpaena madurensis* Cuv. and Val., by Ross F. Nigrelli (1 plate; 2 text-figures); Report of the Hospital and Laboratory of the New York Zoological Park, 1939. Mortality statistics of the Society's collection, by Leonard J. Goss.

THE RABBITS OF CALIFORNIA. *Occasional Papers of the California Academy of Sciences No. XIX.*

By Robert T. Orr. California Academy of Sciences, San Francisco. $3.50. 9⅞ x 6¾; 227; 1940 (paper).

There is a wealth of material dealing with speciation and distribution of rabbits but very little about their habits and life histories. In view of this, the author has devoted over 150 pages of the volume to the life histories, habitats, behavior, food, reproduction, and natural enemies of the seven species, with their subspecies, which are found in California. In addition there are the more usual accounts of specific diagnosis, coloration, distribution, and measurements. The early pages include an account of the techniques employed in the investigation and a discussion of the possible origin of geographic variation. Since the latter is the only section involving speculation, it is the most open to disagreement, particularly by students of evolution and heredity. The author finds that a negative correlation exists between degree of variation and the ability of dispersal, but the reason for the correlation is subject to question. There are several pages of bibliography and numerous photographs.

INTIMATE SKETCHES FROM BIRD LIFE.
By Eric Hosking and Cyril W. Newberry. With an Introduction by Julian S. Huxley. Country Life, Ltd., London. 5s. 8½ x 5½; xx + 104 + 53 plates; 1940.

In this fascinating little book Hosking and Newberry have recorded a number of pertinent observations of bird life in the field. In support of the discussions of various phases of the life and activities of some eight species of English birds are a number of excellent photographs.

The authors have included in their discussions the methods of constructing blinds, the procedures for making careful field observations, and the techniques necessary for successful bird photography. The material of the book is for the most part a record of field observations and objective data, though there are occasional minor excursions into the theoretical with regards to the evolution, the intelligence, and the emotions of birds.

For all nature lovers, particularly those interested in ornithology, the book is well worth the reading time.

AN OUTLINE OF GENERAL ZOOLOGY.
By Gordon Alexander. Barnes and Noble, New York. $1.00. 8¼ x 5⅜; [16] + 280; 1940 (paper).

This is another of the popular college outline series and bids fair to rival the success of the other members of this group of publications. The volume is in four parts: Principles of organization of the animal body, Survey of the animal kingdom, Biological principles, and an Historical summary. This outline is designed to be used as a supplement to standard textbooks used in either of the two kinds of college courses in zoology—the so-called survey and principle courses. The outline may also be used as a syllabus or condensed textbook in courses with a complete lecture plan. The numerous illustrations are well-chosen. The appendix includes an abridged classification of animals and a glossary of important terms. The book is indexed.

ABOUT SPIDERS: *Introducing Arachne.*
By Elaine V. Emans. Drawings by Viola A. Young. Photographs by Lee Passmore and O. C. Kuehn. E. P. Dutton and Co., New York. $2.50. 8 x 5⅛; 183 + 11 plates; 1940.

Not to be too harsh with the writer of this book it must be said that the amateur biologist may gather from its pages many of the facts of spider life. These include home-building, reproduction, the economic importance of the arachnids, and other bits of interest such as that "though probably no cracker has ever been patterned after it, the spider is a little animal, and not an insect." Miss Emans reaches the pinnacle of simplicity and imagination in biological writings. *About Spiders* is an excellent story for children. An effort is made to dispel prejudice against spiders by treating them as household pets whose names are "Beautiful Lady," "Silver Queen," and "Brave One."

AN ATLAS OF THE FROG.
By Paul L. Carroll and Wilfred F. Horner. C. B. Mosby Co., St. Louis. $1.25. 10½ x 7¾; 109; 1940 (paper).

The popularity of the frog as a typical vertebrate for college study has brought into print an abundance of work books, syllibi, and atlases on this common amphibian. The present atlas follows closely the general run of these. It includes labelled drawings of the skeletal, musculature, digestive, respiratory, urogenital, circulatory, and nervous systems.

The loose-leaf form of the atlas conforms to the recent trend in providing the student with a clear, precise and simplified laboratory manual. For college classes that require spoon-feeding in their general biology courses, this atlas should furnish at least part of their pre-digested gruel.

ANIMALS IN ACTION.
By Gayle Pickwell. Whittlesey House, McGraw-Hill Book Co., New York and

London. $4.00. 11¾ x 8¾; xii + 190; 1940.

Each book put out by this author seems to be more fascinating than the preceding one. In this beautiful volume are described and illustrated many interesting phases in the lives of animals—the getting of food, locomotion, protection, breathing, etc. Being a master of photography as well as a careful student of nature the author has produced a work that should stimulate many a young naturalist to follow in his footsteps.

A REVISION OF THE NORTH AMERICAN APHIDS OF THE GENUS MYZUS. *United States Department of Agriculture Miscellaneous Publication No. 371.*

By Preston W. Mason. United States Department of Agriculture, Washington, D. C. 5 cents. 9⅛ x 5⅞; 30; 1940 (paper).

Listed here are 20 species (3 of which are described as new) of the genus *Myzus* Passerini, together with their primary and secondary host, their distribution, their natural history and their economic importance. There is a detailed description of the seasonal variants of each group, a key to the identification of *Myzus*, and many helpful drawings. A bibliography of 35 titles and an index conclude this 30-page bulletin.

TRANSACTIONS OF THE SAN DIEGO SOCIETY OF NATURAL HISTORY, *Vol. 9, Nos. 21, 22, 23 and 24. A New Cardinal from Central Lower California, Mexico*, by Laurence M. Huey; *A New Form of Pocket Gopher (Thomomys) from the Santa Cruz Mountains, California*, by Laurence M. Huey; *A New Coastal Form of Brush Rabbit from the Vicinity of San Quintin, Lower California, Mexico*, by Laurence M. Huey; *A New Species of Legless Lizard from San Geronimo Island, Lower California, Mexico*, by Charles E. Shaw.

Society of Natural History, San Diego, Calif. 10½ x 6⅞; No. 21, 4; No. 22, 2; No. 23, 4; No. 24, 4; 1940 (paper).

UNIVERSITY OF CALIFORNIA PUBLICATIONS IN ZOOLOGY. *Vol. 42, Nos. 8, 9, 10. The Pinyon Mouse (Peromyscus truei) in Nevada, with description of a new subspecies*, by E. Raymond Hall and Donald F. Hoffmeister; *Geographic Variation in Bushy-tailed Wood Rats*, by Emmet T. Hooper; *A New Race of Salamander, Ensatina eschscholtzii picta, from Northern California and Southern Oregon*, by Wallace F. Wood.

University of California Press, Berkeley and Los Angeles. 25 cents each. 10¼ x 6¾; No. 8, 5; No. 9, 18; No. 10, 3; 1940 (paper).

BOTANY

TEXTBOOK OF GENERAL HORTICULTURE.

By Julian C. Schilletter and Harry W. Richey. McGraw-Hill Book Company, New York and London. $5.00. 9 x 6; ix + 367; 1940.

The urgent need in American agricultural colleges for a textbook in general horticulture has furnished the stimulus for preparing this excellent volume. The authors have been aware of the fact that it is practically impossible to organize a general course in horticulture which could be readily used by all types of classes in all regions of the United States, and in consequence, have planned this text around the general concepts and broad principles of the subject, rather than around the specific practices and techniques which vary from place to place throughout the country.

The early chapters of the book deal with horticulture in relation to man's industry, economy, and social well-being throughout the world. There follow several chapters on the structure, function and physiological processes of the horticultural plants, as well as their relation to the environment. The importance of soil management, and the care required for correct propagation, pruning, and training of plants are stressed. The volume closes with several chapters on the control of insect pests and the harvesting and storing of horticultural products.

From this body of well chosen and clearly presented material, it is obvious

that the requirements for different types of classes can be filled. The text will certainly be welcomed in its field.

The inclusion of numerous helpful illustrations, review questions, problems, and suggested collateral reading in each chapter, as well as the detailed index add to the excellence of this fine book.

ELEMENTS OF BOTANY. *Third Edition.* *By Richard M. Holman and Wilfred W. Robbins. John Wiley and Sons, New York; Chapman and Hall, London.* $2.75. 9 x 5¾; xi + 392; 1940.

This abridged text is designed especially for use in institutions in which only one semester is devoted to general botany, or in which the subject is less extensively studied than usual. The first part of the book deals with the structure and physiology of seed-bearing plants, and in the second part the various phyla of the plant kingdom are discussed in order. Considerable attention has been given in this new edition to applications and items of general interest. Mention is made at logical places of such topics as the role of growth substances and their use in agricultural practice, water-culture methods of growing plants, root systems in relation to soil erosion, methods used in altering the duration of the life cycle of plants, artificial pollination, self-sterility in commercial fruit varieties, economic importance of plant diseases, control of weeds, "hardening" of plants, "short-day" and "long-day" plants, economic value of certain plants and of various plant products, and methods employed to induce chromosome changes. The book is well illustrated. There is no bibliography, but a good index is provided.

FOREST OUTINGS. *By Thirty Foresters. Edited by Russell Lord. United States Department of Agriculture, Washington, D. C.* $1.25 (cloth); 75 cents (paper). 10¼ x 7¼; xiv + 311; 1940.

The pleasures, romances, and healthful modes of recreation that our national forest parks offer are here recounted in a most interesting fashion by 30 foresters. There is also an account of the administration of our 161 "monuments to nature" in an attempt to make and keep them as "zoos without cages." The discussions deal with such diversified subjects as camps, winter and summer sports, dangers from fires and floods, and the conservation of timber, soil, and wild life. The generous supply of excellent photographs are enticing bait to lure the reader into the more worthwhile things in the text.

A short bibliography, a note on the basic principles governing the recreational management of the national forests, a map and several tables indicating the location and size of the national parks, a table of the big game census of the national forests as of January 1, 1939, and an index complete the volume.

BOTANY OF THE MAYA AREA. *Miscellaneous Papers XIV–XXI. Publication 522. Carnegie Institution of Washington, Washington, D. C.* $2.75 (cloth); $2.25 (paper). 10 x 6⅞; [4] + 474 + 7 plates; 1940.

This comprehensive survey of the vegetation in the Maya region, involving extensive systematic studies, contains the following papers: The 1936 Michigan-Carnegie Botanical Expedition to British Honduras (four plates and one map), by C. L. Lundell; The apocynaceous flora of the Yucatan Peninsula (six text figures), by R. E. Woodson, Jr.; The Bromeliaceae of the Yucatan Peninsula (twenty text figures), by L. B. Smith and C. L. Lundell; The Eriocaulaceae, Verbenaceae, and Avicenniaceae of the Yucatan Peninsula, by H. N. Moldenke; The Labiatae of the Yucatan Peninsula, by Carl Epling; The sedges of the Yucatan Peninsula (three text figures), by Hugh T. O'Neill; The Melastomaceae of the Yucatan Peninsula, by H. A. Gleason; The Bignoniaceae of the Maya area (three plates), by R. J. Seibert. The subject matter is presented in great detail, each paper concludes with an index to cited specimens and at the end

of the volume is an index to publications 461 to 522 in this series.

PHYSIOLOGICAL STUDIES OF JERUSALEM-ARTICHOKE TUBERS *with Special Reference to the Rest Period. United States Department of Agriculture: Technical Bulletin No. 657.*

By Clarence E. Steinbauer. United States Department of Agriculture, Washington, D. C. 15 cents. 9⅛ x 5¾; 52; 1939 (paper).

The importance of the Jerusalem-artichoke (*Helianthus tuberosus* L.) as a possible source of levulose and of food for diabetics has been realized only in the past few years. In this bulletin, the author has recorded an enormous quantity of data concerning the chemical analysis of the Jerusalem-artichoke tubers both in the growing and resting stages. The study includes also the effects of different temperatures and chemical treatments in storage on the length of the resting period and the time required for sprouting. A storage temperature of 32° C. seems to be conducive to the shortest rest period and the best sprouting. None of the chemicals used produced any noticeable effect on speed of sprouting.

The paper includes an extensive review of the literature, as well as numerous photographs, charts and tables.

TREES.

By Samuel R. Stevens. Cecil Baugh and Company, Dallas, Texas. $3.00. 9 x 6; xiii + 201; 1940.

The value of trees in the complex economy of man's life is the central theme of this intriguing little book. Supporting this theme are such topics as the history of the saw and sawmilling, sign language used in sawmills, lumber grading, the process of drying and the shrinkage of lumber, conservation and reforestation, as well as a discussion of many famous trees in history. Throughout the text there is a plea for the conservation of trees and for a more thorough program of reforestation, not only for industrial purposes, but simply for the beauty and the grandeur of the tree as such. Scientific readers will be annoyed by the author's constant use of the word "specie" when describing a single species.

The book is well illustrated and documented, but there is no index.

FLORA HAWAIIENSIS. *The New Illustrated Flora of the Hawaiian Islands. Book 4.*

By Otto Degener. (Obtainable from the author, Waialua, Oahu, Hawaii). $3.50. 9⅜ x 6¼; 330; 1940.

This is Book 4 of a comprehensive work that is being published at irregular intervals in loose-leaf fasciles (Centuries), each book containing 100 descriptions and 100 full-page illustrations. The arrangement is such that as each new book appears the pages can be taken out and placed with the old in taxonomic sequence. The *Flora* is planned for the student and nature-lover but it will be found a useful guide by Hawaiian visitors who are interested in the luxuriant vegetation of the islands.

FUNDAMENTALS OF BACTERIOLOGY. *Second Edition, Revised.*

By Martin Frobisher, Jr. W. B. Saunders Co., Philadelphia and London. $4.00. 7¾ x 5⅜; xvii + 653; 1940.

The second edition of this text is welcome (for mention of first edition cf. Vol. 12, p. 373). The author has revised his book with the idea in mind "that there is no longer any reason ..... to regard bacteriology as an exclusively medical science." This could well be a starting-point for a consideration of the fundamentals of any biological science. History, principles, technique, mode of action, and the industrial and medical aspects of microbiology are discussed.

ELEMENTARY BACTERIOLOGY. *Fourth Edition, Revised.*

By Joseph E. Greaves and Ethelyn O.

Greaves. W. B. Saunders Co., Philadelphia and London. $3.50. 7¾ x 5⅜; xiv + 587; 1940.

This is the fourth edition of a textbook for an introductory course in bacteriology. The previous editions as they appeared have been reviewed in the Q.R.B. The material is presented in a style such that the person whose chief qualification for admission to the course is his interest in the world around him, will find his way to a better understanding of disease and its control. The many illustrations are helpful.

APPLIED MYCOLOGY AND BACTERIOLOGY.

By L. D. Galloway and R. Burgess. Chemical Publishing Co., New York. $4.00. 8⅝ x 5⅜; viii + 186; 1940.

This edition brings up to date a book first published in 1937. New material and illustrations have been added, and the bibliography has been expanded to include recent literature. As before, the emphasis is on the economic value of microbiological techniques.

MORPHOLOGY

THE ENAMEL OF HUMAN TEETH: *An Inquiry Into the Formation of Normal and Hypoplastic Enamel Matrix and Its Calcification.*

By Moses Diamond and Joseph P. Weinmann. Columbia University Press, New York. $1.50. 10¾ x 7¾; viii + 105; 1940 (paper).

Retzius's concept that human tooth enamel is formed from within outward and that the organic matrix is calcified progressively as developed has been generally accepted during the past century. The present authors here show that this concept must be expanded. They separate the development of tooth enamel into two processes—the formation stage "during which the adult thickness of enamel is formed in a matrix state with calcium salts contained therein, probably in colloidal form," and the calcification state "which is regarded as a crystallization phenomenon of the colloidal calcium." Their evidence is based on chemical studies and photomicrographs of enamel matrix formation. Hypoplasia and tooth injuries are treated to some extent but the conclusions with respect to these seem to be based on too little evidence. This book is profusely illustrated, and there is a bibliography of 36 titles.

PHYSIOLOGY AND PATHOLOGY

MEDICAL WORK OF THE KNIGHTS HOSPITALLERS OF SAINT JOHN OF JERUSALEM.

By Edgar Erskine Hume. Foreword by His Most Eminent Highness Fra Ludovico Chigi-Albani. Preface by Lieut.-General Sir Aldo Castellani. The Johns Hopkins Press, Baltimore. $3.00. 10 x 7; xxii + 371; 1940.

This is the first comprehensive account in English of the accomplishments of that remarkable organization, the Knights Hospitallers of Saint John of Jerusalem. The author has performed his task with meticulous care. Having a knowledge of languages he has been able to comb many sources for his material and the result is an accurate and most interesting story of the medical and charitable work of the Order and its historical background.

Three periods are given: (1) from the foundation of the Order in Jerusalem in the latter half of the 11th century, and the establishment of other hospitals throughout Christendom, to the occupation of the island of Malta in 1530; (2) the period of occupation of Malta (1530–1798); (3) from the loss of Malta to the present (1798–1940). From the very early times there were Sisters of the Order as well as Knights, and both sexes (Mohammedans, Jews, and Christians alike) were received as patients, including abandoned infants. Of much interest are the rules and regulations concerning sanitary arrangements, food, and behavior (of both patients and nurses). Of the size of the first hospital to be established, a visitor to Jerusalem prior to 1187 writes:

> Here on the south side of the church, stands the Church and Hospital of St. John the Baptist. As for

this, no one can credibly tell another how beautiful its buildings are, how abundantly it is supplied with rooms and beds and other material for the use of poor and sick people, how rich it is in the means of refreshing the poor, and how devotedly it labors to maintain the needy, unless he has had the opportunity of seeing it with his own eyes. Indeed, we passed through this palace, and were unable by any means to discover the number of sick people lying there; but we saw that the beds numbered more than one thousand.

The volume is beautifully printed and illustrated and concludes with a list of references and a carefully planned index.

THE CHINESE WAY IN MEDICINE.
By Edward H. Hume. The Johns Hopkins Press, Baltimore. $2.25. $7\frac{3}{4}$ x $5\frac{1}{2}$; [8] + 189; 1940.

This book tells in a clear-cut way the story of Chinese medicine from earliest records on through the centuries. Reduced to fundamentals, Chinese medical practice depended for centuries largely on the effect of mind on matter, and this enemy of disease having been exploited to the fullest extent, nature was then called in to do the rest. The Chinese did, however, resort early to the use of herbs, animal products, and minerals in the treatment of disease, and many of these are now of recognized value by the medical profession throughout the world. Hua T'o, in the second century, discovered the use of anaesthetics and became skillful as a surgeon. He gave his patients

an effervescing powder in wine which produced such numbness and insensibility that he could open abdomen or back, as the case might be, and wash, cut, or remove diseased organs. He would apply sutures, then cover wounds with a salve, and count on their healing completely in four or five days, the tissues returning to normal within a month.

The *Pulse Classic*, in ten volumes, one of the standard works in clinical medicine, was written in the third century. Hume says that proficient Chinese physicians of the old school show an almost uncanny power in diagnosing organic diseases by pulse observations alone. But it is only after years of experience that this art is acquired, the procedures being far more complicated than merely noting the rate, the volume, and the tension.

The volume is thoroughly documented and there is an index.

СТАРОСТЬ (Труды Конференции по Проблеме Генеза Старости и Профилактики Преждевременного Старения Организма). Киев, 17-19 Декабря 1938 г.
А. А. Богомолец, Редактор. Издательство Академии Наук УССР, Киев. 245 x 163; 492; 1939.

[OLD AGE. (*Proceedings of the Conference of the Problem of the Development of Old Age and the Prevention of Premature Aging of the Organism*), *Kiev, December 17–19, 1938. Edited by A. A. Bogomolets. Publishing Agency of the Ukrainian Academy of Science, Kiev.* $9\frac{3}{4}$ x $6\frac{1}{4}$; 490; 1939.]

The Conference of the Problem of the Development of Old Age and the Prevention of Premature Aging of the Organism was a joint enterprise set up by the Institute of Clinical Physiology of the Ukrainian Academy of Science, the Institute of Experimental Biology and Pathology of the Commissariat of Public Health, and the Institute of Clinical Medicine of the Commissariat of Public Health, all of the Ukrainian Soviet Socialist Republic. The thirty-nine papers presented at the meetings are published in this volume. Many of these are based on experimental material on physiological and biochemical processes and changes in old age. S. Tomeline has contributed a paper on the statistics of persons over sixty in various countries. French summaries have been provided. The bibliography is divided into the following divisions: works in the Russian and Ukrainian languages and translations from these (171 items); books and monographs in languages other than Russian or Ukrainian (117 titles); publications in periodicals other than Russian or Ukrainian, subdivided by subject (738 titles).

RHEUMATIC FEVER. *Studies of the Epidemiology, Manifestations, Diagnosis, and Treatment of the Disease during the First Three Decades.*

By May G. Wilson. Commonwealth Fund, New York; Humphrey Milford, London. $4.50. 10 x 6½; xiv + 595; 1940.

Rheumatic fever, under which title are included acute polyarthritis, chorea, and rheumatic carditis, still remains today one of the most obscure diseases or syndromes. As a clinical entity its boundaries are not very well defined, its etiology is unknown and the part played by contagion and by heredity in its transmission is controversial. The author has devoted considerable time to the investigation of these questions and in this volume summarizes her clinical and research experience as well as the more recent observations reported by others. In the first of five parts the epidemiology and etiology of the disease are discussed. The second part concerns the clinical and pathological manifestations of the disease, the third is devoted to its course in the first three decades of life, the fourth to the diagnosis of rheumatic carditis, the fifth part to therapy of rheumatic children. Although it is clearly evident that neither the efforts of the author nor those of other investigators have succeeded in adequately clarifying the fundamental questions concerning rheumatic fever, this orderly and comprehensive treatment of the findings at hand provides the necessary background for further studies on the subject.

TUBERCULOSIS AND GENIUS.

By Lewis J. Moorman. University of Chicago Press, Chicago. $2.50. 9 x 5½; xxxv + 272 + 10 portraits; 1940.

The author apparently believes that some causal relationship exists between tuberculosis and the manifestations of genius, artistic genius particularly. However, in this book he does not seek to demonstrate this relationship but desires merely to reveal that tuberculosis is no obstacle to creative work. Therefore, he has compiled short biographies of 10 famous persons who probably had active clinical tuberculosis, or so the author believes. The biographies concern Robert Louis Stevenson, Schiller, Marine Bashkirtseff, Katherine Mansfield, Voltaire, Molière, Francis Thompson, Shelley, Keats, and Francis of Assisi. In the elaboration of these biographical sketches the author has given free rein both to his imagination and to his knowledge of the course of tuberculosis. It would seem as if on the framework of the subject's life history the author has superimposed a typical tuberculosis case history, the same for all. An attempt is made to demonstrate that the infection was acquired in childhood from familial sources and that periods of exacerbation of the disease accompanied periods of great intellectual efforts. The style of writing resembles that of the Victorian romanticists so that, all in all, this book is definitely not recommended as a gift for sick friends.

PROGRESS IN MEDICINE. *A Critical Review of the Last Hundred Years.*

By Iago Galdston. With a foreword by Henry E. Sigerist. Alfred A. Knopf, New York and London. $3.00. 8½ x 5½; ix + 347 + xiv; 1940.

This is not just another "romance" of the history of medicine revolved around the lives of a few trail-blazers. Rather it is a history of the ideas and philosophy underlying the great discoveries of bacteriology (the advance of the microbe theory of disease, and its fruits, the use of antiseptics, vaccines and serum, and modern sanitation), endocrinology, nutrition and the vitamins, and psychiatry which have been developed in the past century. Side-lights on the lives of the personalities in these fields (Pasteur, Koch, Erlich, Claude Bernard, Mesmer, Brown-Séquard, Braid, Charcot and Freud among a host of others), and the stories of how they came to think of this or observe that, add interest. Galston writes with clarity and dignity and there is not a dull line in the book.

PREVENTIVE MEDICINE. *Sixth Edition, Revised.*

By Mark F. Boyd. W. B. Saunders Co.,

Philadelphia and London. $5.00. 9⅛ x 6; xi + 588; 1940.

Boyd's book represents an endeavor to present briefly the salient features of modern preventive medicine. The material is presented in eight sections as follows: Diseases due to invading microorganisms, Deficiency diseases, Occupational diseases, The puerperal state, Heredity and disease, Special aspects of hygiene and sanitation, Demography, and Public health. Since the last edition (cf. Q.R.B., Vol. 12, p. 125), rapid advances in knowledge of preventive medicine have taken place. In this new edition, revision in the chapters treating of sewage disposal, water supply, and deficiency diseases is particularly noteworthy. Much new material has been added to the sections dealing with syphilis, meningitis, pneumonia, tuberculosis, and yellow fever. An extensive bibliography is placed in the appendix and the book is completely indexed.

DISEASES OF WORKERS. *The Latin text of 1713 Revised, with translation and notes. De Morbis Artificum. Bernardini Ramazzini Diatriba.*

By Wilmer C. Wright. University of Chicago Press, Chicago. $5.00. 9¼ x 6; xlvii + 549; 1940.

This is the seventh and one of the most interesting of the series of classical medical works republished under the auspices of the Library of the New York Academy of Medicine. Ramazzini's *opus*, aside from its historical value as the first systematic treatise on industrial hygiene, reflects the status of the medical art and science of his period. In addition, a first hand description is obtained of the working conditions and techniques of the 17th and 18th centuries. The translation is very readable and remarkably accurate. Included is a short biography of Ramazzini and the bibliographic references pertaining to him.

A TEXTBOOK OF PHYSIOLOGY. *Fourteenth Edition, Thoroughly Revised.*

By William H. Howell. W. B. Saunders Co., Philadelphia and London. $7.50. 9¼ x 6; xix + 1117; 1940.

Few texts with as long and distinguished a career as this one has had—it is now in its 35th year and 14th edition—have had the good fortune to be revised by the original author. The book has lived through a period of rapid development and great attainment in physiological research, and in preparing the present edition Howell says that it has been necessary to supplement "his own reading of original sources by frequent references to the many excellent reviews and summaries published by specialists in their several fields." In every way the volume maintains the high standard of previous issues.

OBSERVATIONS MADE DURING THE EPIDEMIC OF MEASLES ON THE FAROE ISLANDS IN THE YEAR 1846.

By Peter Ludwig Panum. Translated from the Danish by Ada Sommerville Hatcher. With a Biographical Memoir by Julius J. Petersen. Translated from the Danish by Joseph Dimont. And an Introduction by James Angus Doull. American Public Health Association, New York. 8 x 5½; xxxvii + 111; 1940.

This well-annotated translation of Panum's report is fittingly dedicated to the late Wade Hampton Frost. In his courses on epidemiology at the Johns Hopkins School of Hygiene and Public Health, Frost would always review this paper as an example of careful and systematic ordering and interpretation of facts. Because of these qualities the book certainly deserves to be brought to the attention of students of medical sciences. Included in appendices are also the reports of Dimont and of Manicus, respectively, on the same epidemic.

THE VIRUS: *Life's Enemy.*

By Kenneth M. Smith. The Macmillan Co., New York; The University Press, Cambridge. $2.00. 7½ x 5; viii + 176 + 10 plates; 1940.

This volume in the *Cambridge Library of Modern Science* makes available to the general reader information of the greatest practical value which reads like a first-rate adventure tale. The history of virus study, the nature of viruses, and their mode of action are discussed in simple language. The author's treatment of the subject is authoritative, yet it is as concise and as interesting as a reporter's column, and as timely as newspaper accounts of the recent west-coast influenza epidemic.

RECENT ADVANCES IN SEX AND REPRODUCTIVE PHYSIOLOGY. *Second Edition.*

By J. M. Robson. With an Introduction by F. A. E. Crew. The Blakiston Co., Philadelphia. $5.00. 8 x 5¼; xiii + 329; 1940.

As in the first edition (cf. Q.R.B., Vol. 9, p. 489) the emphasis is placed on the hormones. In the past six years the progress in this field has been so vast that considerable changes and additions have been necessary. This edition is 80 pages longer than the first. New chapters include consideration of the properties of the male hormone and other androgens, the chemistry of the gonadic hormones, and the methods used in the standardization of the sex hormones. A selected bibliography has been added to each chapter, with those items which contain a detailed review marked with an asterisk. The volume is illustrated, and a subject index has been provided.

THE EMPEROR'S ITCH. *The Legend Concerning Napoleon's Affliction with Scabies.*

By Reuben Friedman. Froben Press, New York. $1.50. 9⅛ x 6; 82 + [8]; 1940.

Everyone who is familiar with the "Napoleonic pose" has his own theory, serious or otherwise, as to why the French emperor so frequently had his hand inside his shirt-front. In this little book an authority on some of the reasons why men —and women too—itch, gives his conclusions on the Little Corporal's physical troubles. His evidence, though necessarily circumstantial, is good, and the diagnosis (dermatitis herpetiformis) he makes fits the picture historians have drawn of the dynamic, almost neurotic, conqueror.

BIOCHEMISTRY

TEXTBOOK OF BIOCHEMISTRY. *Second Edition, Revised.*

By Benjamin Harrow. W. B. Saunders Co., Philadelphia and London. $3.75. 9¼ x 6; ix + 439; 1940.

In this book the story of biochemistry is told in the form of closely-knit chapters. It covers the usual requirements of courses offered to medical, dental, agricultural, and general college students. Among the new material incorporated in the present edition is: Stanley's work on mosaic-diseased tobacco plants, Northrop's purification of bacteriophage, the multiple nature of vitamin A, the story of vitamin K and blood coagulation, sulfanilamide and sulfapyridine, the use of the nitrogen isotope in the study of protein metabolism, Kögl's work on d-glutamic acid and tumor tissue, newer conceptions of coenzymes and carriers in biological oxidation, and the chemistry of pantothenic acid, vitamin B_6, and vitamin E. The extensive references are placed at the end of each chapter. The emphasis has been placed upon reviews rather than on the original papers, though the latter are by no means excluded. Preference has been given to articles in English. An appendix giving in tabular form the nutritive value of foods, and a complete index conclude this excellent volume.

THE CHEMISTRY AND TOXICOLOGY OF INSECTICIDES.

By Harold H. Shepard. Burgess Publishing Co., Minneapolis. $4.00. 10¾ x 8½; iii + 383; 1940.

It is the avowed purpose of the author to present in this book the important facts and theories relative to insecticides,

including not only the chemical, physical and toxicological aspects, but also information regarding the history and commerce pertaining to this field. This text furnishes an excellent guide to the insecticide literature. Methods of insect control are not given except as the action of specific insecticides is illustrated. In addition to chapters on the history of insecticides, principles of insecticide toxicology, and quantitative toxicology of insecticides, stomach poisons, contact insecticides, plant derivatives and related compounds, insect fumigants, and miscellaneous chemical control are discussed in order. The volume is intended not only for college students, but for the many persons in public and private entomological pursuits who need a reference book on this subject. Extensive lists of references are given throughout the book and a good index is provided.

PROXIMATE COMPOSITION OF AMERICAN FOOD MATERIALS. *U. S. Department of Agriculture. Circular No. 549.*

By Charlotte Chatfield and Georgian Adams. Government Printing Office, Washington. 15 cents. 9⅛ x 5¾; 91; 1940 (paper).

The formulation of a diet program requires a reasonably accurate knowledge of the composition of the various articles of food. This publication of the U. S. Department of Agriculture supplies information that is desirable, and presents in concise form an analysis of a large number of foods in general use in the United States of America. One table classifies fruits and vegetables according to the percentage of carbohydrates they contain. A second table includes an analysis of meats, fish, fruits, vegetables, cereals, cereal products, and miscellaneous products, showing the percentage of refuse, water, protein, fat, ash, carbohydrates, and acid they contain, and their fuel value (calories per 100 grams and per pound). There are also citations of literature containing more detailed and technical information as to variability in composition of various food products. Dietitians will find this publication useful.

VITAMIN E. *A Symposium held under the Auspices of The Food Group (Nutrition Panel) of the Society of Chemical Industry on Saturday, 22nd April, 1939 at the School of Hygiene and Tropical Medicine, Keppel Street, London, W.C. 1, England.*

Edited by A. L. Bacharach and J. C. Drummond. Chemical Publishing Co., New York. $2.00. 8½ x 5⅜; viii + 88; 1940.

As a spur to the pursuit of further knowledge, a symposium on vitamin E was held, April 22, 1939, at the School of Hygiene and Tropical Medicine in London, under the auspices of the Society of Chemical Industry. Fifteen papers and the discussions which followed their presentation are published in this monograph, which represents the proceedings of the conference. The meeting was a collaboration between workers in pure and in applied science, as the joint editorship of this volume by A. L. Bacharach of the Glaxo Laboratories, and J. C. Drummond of University College attests.

INVESTIGATIONS ON THE PHYSICAL AND CHEMICAL PROPERTIES OF BEESWAX. *United States Department of Agriculture: Technical Bulletin No. 716.*

By Charles S. Bisson, George H. Vansell, and Walter B. Dye. United States Department of Agriculture, Washington, D. C. 5 cents. 9⅛ x 5¾; 24; 1940 (paper).

This thorough investigation of the properties of crude beeswax indicates that the properties of the impurities within the wax and not those of the wax itself, must be used for purposes of classification. The analysis of some 60 samples of crude wax included the determination of the melting point, the solidifying point, the density at 20° C., the refractive index at 80° C., the ash content, the saponification number, the acid number, the ester number, the ester-acid ratio, and the iodine number. Experiments dealing with the effect of metals on the color of waxes indicate that from the standpoint of cost and practicability, aluminum equipment is the most desirable for handling waxes. The value of sunlight in bleaching waxes is worthy of commercial application.

The text is well supplied with tables

and graphs, and is well documented in support of these significant findings.

SEX

SEX IN DEVELOPMENT. *A study of the growth and development of the emotional and sexual aspects of personality together with physiological, anatomical, and medical information on a group of 153 normal women and 142 female psychiatric patients.*

By Carney Landis and Coauthors. Foreword by Nolan D. C. Lewis. Paul B. Hoeber, Medical Book Department of Harper and Bros., New York and London. $3.75. 9¼ x 6; xx + 329; 1940.

Psychological investigations made by the questionnaire method on 154 "normal" and 142 psychotic or neurotic women form the basis of this analysis of the growth and development of emotional and sexual patterns in an attempt at an evaluation of the importance of psychosexuality in psychopathology. Anatomical, physiological, and medical findings for these same women were correlated with the psychological. Interpretive discussions accompany the case histories used to illustrate the normal sexually developed woman, the psychosexually immature woman, the homoerotic woman, the unhappily married woman and the psychiatric patient. A very wide range of variation in experience, emotion, and growth was apparent when an attempt was made for the construction of a composite biography of the course of normal psychosexual development. Few anatomical or physiological differences were noted between the normal group and the mentally disordered women, and therefore the definitely inferior sex adjustment in marriage in the latter group is assumed to be related "to certain unspecified psychological factors associated with the mental disease itself."

Although criticism may be made that many important aspects of the problem were not, and could not be, considered, due to the interview technique used, this is a definite contribution to the subject and an impetus for further research.

The volume concludes with four appendices containing the information forms used in the study, vital statistics, evaluation scales, and interscale relationships, respectively, a bibliography, an index of authors, and one of subjects.

REPORT ON THE SEX QUESTION.

By The Swedish Population Commission. Translated and Edited by Virginia C. Hamilton. Published for National Committee on Maternal Health, Inc. by The Williams & Wilkins Company, Baltimore. $2.00. 8 x 5⅜; xx + 182; 1940.

This frank and calm scientific study of the sex question in Sweden may well serve as a model for similar studies in other countries that are following Sweden's lead in a decline in the birth rate. It is an abridged translation of the original report, and appendices which dealt with "special investigations of certain aspects of the material" have been omitted. Nevertheless it is well worth study by all students of population problems. In Sweden the declining birth rate has become a serious problem and has proceeded farther than in most other countries. The Commission emphasizes that the direct cause "must be sought in the sex life of the individual." To this end the main part of the report is devoted to the question of the extent of contraceptive practice and its evaluation from practical viewpoints—eugenic, medical and hygienic, psychological, economic, and demographic. Extramarital sex relationships, the extent of venereal diseases and the campaign against them, and prostitution are also considered. The report definitely demonstrates that the increase in the intentional practice of birth control (this term includes preventive birth control and induced abortion) "is the main, if not the only, responsible factor." Proposals for reorganizing sex instruction in the schools, and recommendations, which do not reek of propaganda, for a population program are made.

A Short History of Sex-Worship.

By H. Cutner. Watts and Co., London. 8s. 6d. net. 8½ x 5½; xiii + 222; 1940.

According to the author religions in their origins were closely connected with the mysteries of reproduction and fertility in man and nature, either alone or in combination with sun-worship. Probably the sex element in religion reached its height (or degradation) in the phallic cults which were curious though dominating elements in many ancient religions. These rites are still practiced today in one form or another among primitive peoples in various parts of the world, and in India. Cutner claims also that some of the Christian rites and symbols may be traced to a phallic origin—for example the *fish*, one of the favorite symbols of Christ was also a symbol of Venus—and in the freak sects that have sprung up from time to time in Europe and the United States the penchant for sex or its repression plays a prominent part. In this book the author provides the reader with historical information which is not easily accessible to the general public, for obvious reasons. The bibliography is confined to a few titles of books and there is an index.

BIOMETRY

A Detailed Proof of the Chi-Square Test of Goodness of Fit.

By E. Russell Greenhood, Jr. Harvard University Press, Cambridge. $1.25. 6¾ x 4⅜; xii + [2] + 61; 1940.

The author states that, "the aim of this paper is to try to present a clear proof that will bridge the gap between what is known as the chi-square test of goodness of fit and the actual mathematical curve, the chi-square curve." He admits to the feeling, as many students in elementary statistics have, that something was being put over on him in regard to proof of the aforementioned test. The answer was not to be found in R. A. Fisher's *Statistical Methods for Research Workers*. Here he found only precise statements upon how to apply the test. The paper by Pearson, who originated the test, was found to be little short of occult. It was only on reading T. C. Fry's article in the *Journal of the American Statistical Association*, Sept., 1938, that a crack of light appeared in the stygian void. With Fry's paper as a lever the world of chi-square was moved by the writer and the chi-square equation derived.

The lively, informal style of the work is very unusual in a statistical treatise and for this reason very pleasing. The interpretation to be placed upon "P" is discussed. A good bibliography is appended.

Statistical Procedures and Their Mathematical Bases.

By Charles C. Peters and Walter R. Van Voorhis. McGraw-Hill Book Co., New York and London. $4.50. 9 x 6; xiii + 516; 1940.

Many persons using statistical analyses in their work have often felt the need of a knowledge of the development of the formulas used. This volume was written to bridge the chasm between elementary courses, in which the formulas are given from a purely authoritative viewpoint, and the original papers, which are often highly mathematical in nature. The writers have produced a useful book.

The opening chapter deals with some of the elements of calculus. The following divisions treat of: measurement of central tendencies, variability, rectilinear correlation, the reliability of statistics, partial and multiple correlation, the normal curve, the chi-square test, curve fitting, and the technique of controlled experimentation.

Every chapter has appended a number of exercises and reference for further study. Several statistical tables are present at the end of the book and there is an index.

The Bulletin of Mathematical Biophysics. *Volume 3, Number 1, March, 1941. Edited by N. Rashevsky. University of Chicago Press, Chicago.*

This number contains the following

papers: Some remarks on the movement of chromosomes during cell division, by N. Rashevsky; On reinforcement and interference between stimuli, by G. Young; Studies in the mathematical biophysics of discrimination and conditioning I, by H. D. Landahl; Cellular forms and movements, by A. S. Householder.

PSYCHOLOGY AND BEHAVIOR

DYNAMICS IN PSYCHOLOGY.

By Wolfgang Köhler. Liveright Publishing Corp., New York. $2.50. 8¼ x 5½; [6] + 158; 1940.

The nineteenth century psychologists generally considered the field of their investigations as discontinuously separated from the rest of nature. This attitude tended to give them more intimate contacts with the quack element among the phrenologists than with the legitimate biological sciences. In the twentieth century a normal reaction set in, and the value of the biological sciences as the only possible foundation on which a truly scientific psychology might be erected began to be appreciated.

Köhler swings the pendulum vigorously to the other extreme. Not unmindful of the importance of biology, he goes beyond it to the physics of Faraday and Clerk-Maxwell for fundamental principles. The result is that his book suffers from the attempt to crowd too much material into too small a space, to pack too many thoughts into too few words. For instance, many times the author describes experiments and from the observed results draws conclusions which sometimes seem irrelevant or even contradictory. These unsatisfactory deductions might have been clarified had the author set out a few more of the links in his chain of ratiocination.

Probably most readers will find the most stimulating part of the book to be the discussion in the final chapter of the place of theory in science. Today the emphasis in much scientific research seems to be placed chiefly on the accumulation of facts, rather than on their classification and interpretation. Perhaps this is due to the fear of humiliation that an author must feel when a theory which he has formulated is forced to give ground to a subsequently verified fact that stubbornly stands in its way. Köhler evidently believes that a scholar manifests his character best not by the tenacity with which he maintains a theory but by the readiness with which he accepts modification to it to bring it into agreement with changing conditions. He ably argues the case for scientific theory and points out that the greater scientists are characterized by a sense of humility and reverence such that he is not discouraged by the thought that his creative achievements (and theory is the only creative achievement of science) must of necessity undergo modification as the result of the discovery of new facts.

The author has assumed the rôles of Aaron and Hur to the theoretic biologist's Moses, for which the theoretic biologist has every reason for gratitude.

CONDITIONING AND LEARNING.

By Ernest R. Hilgard and Donald G. Marquis. D. Appleton-Century Company, New York and London. $2.75. 8 x 5¼; xi + 429; 1940.

This highly technical survey of the investigations of Pavloff and his satellites covers the entire field of experimental psychology in a thoroughly commendable manner. From it we learn that Pavloff never considered himself a psychologist; he was a specialist in neurology. But the influence of the experimental technique which he developed defies mensuration, for it is difficult to imagine how the researches of such outstanding figures in experimental psychology as Bekhterev, Watson, or Yerkes could have been initiated without Pavloff to point the way. The procedure by which rats are taught to emulate Ariadne, or chimpanzees to pile Ossa on Pelion, while not exactly homologous to that by which dogs can be made to salivate, is somewhat similar, and the reactions may be conditioned in somewhat the same way. All this receives detailed treatment in this comprehensive compendium.

The authors state that in 1915 Watson lectured on respiration and heart rate. Unfortunately they do not discuss these instances of conditioning, for the effect that such might have on philosophic thought is quite considerable.

The work here under discussion has a subject index of ten and one-half pages, and a bibliographic index of 973 separate items, but its most helpful feature is a twelve-page glossary—not an ordinary glossary, but one that contributes vitally to the reader's comprehension of the book.

THE TREE OF LIFE *of the Human Personality*. *By Mary Macaulay. C. W. Daniel Company, London.* 2s. 8½ x 5½; 29 + 2 folding plates; 1940 (paper).

The frontispiece of this pamphlet is a design of a tree representing the positive aspects of the psychological development of man from babyhood, through childhood and adolescence, to full maturity; at the back is one showing the negative aspects. These are so placed that they may be turned out for consultation while reading the interpretations which form the subject matter. Among the conclusions we find:

> It may be said . . . that the endless conflicts centering round the question of personal liberty come mainly from *abuses in the nursery*. The incoming personality of little children is seldom treated with due respect, or its innate dignity scrupulously maintained. A common cause of early loss of self-respect and self-confidence is a cruelly restrictive material environment, in other words, dire poverty. Parental attitudes of possessiveness, and these, too, are general in all countries, violate personal liberty from the moment of birth (if not before). Parental and social ignorance of, and indifference toward, children's psychological needs. . . . These conditions produce unbalanced people, tyrants, snobs, bullies and aggressors and the multitude of their too docile unheroic victims or followers.

The above quotation serves also to illustrate the style of writing.

EFFECTS OF QUANTITATIVE VARIATION OF FOOD-INCENTIVE ON THE PERFORMANCE OF PHYSICAL WORK BY CHIMPANZEES. *Comparative Psychology Monographs, Volume 16, Number 3, Serial Number 82. By Frank M. Fletcher. The Johns Hopkins Press, Baltimore.* 75 cents. 10 x 6⅞; 46; 1940 (paper).

This investigation was undertaken to determine the quantitative relationship between size of incentive and the work the chimpanzee would do to obtain it. Among the results obtained were:

> All measured characteristics of pulling-in behavior were significantly affected by the size of the incentive. . . . With a certain constant resistance (which varied from subject to subject), frequency of response increased from 0 per cent to 100 per cent with increase in size of the incentive. . . . The results indicate that in a work situation a chimpanzee tends to develop a fairly constant method of pulling with a given kind and amount of resistance, and if a food-incentive is drawn in at all, it is pulled in this way.

Large individual differences were found to exist among the four animals used. Various factors, such as number and size of other incentives presented in close temporal proximity, training, satiation, fatigue, food-hunger, etc. affected the relationship of size of incentive and resistance level to frequency of response. A short bibliography is appended.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

THE SOCIAL MIND: *Foundations of Social Philosophy. By John Elof Boodin. The Macmillan Co., New York.* $3.50. 8 x 5⅜; xi + 593; 1940.

Boodin's book, the product of a life-time of study and thought, is far too complex to be adequately dealt with in a brief review. It is a book for the student and the intelligent lay reader, if he is willing to devote the time necessary to grasp its import. The thoughtful reader, once well within its pages, will find it absorbingly interesting.

Basic in the author's writings since early in the present century are the two concepts, creative synthesis (epigenesis, emergence), and wholism or gestaltism. For the benefit of those who have not been following modern philosophical

thought we give Boodin's clear but concise definitions of these terms:

> Creative synthesis (epigenesis, emergence) means that new characters, not present in the constituent factors or conditions, appear as a result of the interaction. Wholism means that individuals or events can be understood only as figuring in a whole or gestalt. It is important to bear in mind that this whole must have reference to time as well as to space. Social relations illustrate both emergence and wholism. The two concepts, however, do not always go together. Casual social relations may influence human beings, but they do not constitute a whole or group.

Psychology in the past has started with the idea of isolated minds and from this postulate an attempt has been made to comprehend how one mind can understand and evaluate another by means of analogical inference. The author believes that "we must start with the postulate of intersubjective continuity [the immediate consciousness of mental responsiveness] as an elementary fact." Mind, like matter, must be conceived as existing in fields with their own continuities and their own play of parts.

It is pointed out that social minds must be real if they possess characteristics analogous to those of particular minds. One of these important characteristics to which the author gives much attention is *fusion*. In social fusions, the quality of the components makes a difference. The French fusion is different from the Anglo-Saxon fusion; the feminine fusion differs from the masculine fusion. Social minds are far more numerous than personal minds since "social continuities intersect individual centers in an indefinite number of planes."

A careful reading of the section on "The existence of the social mind" will well repay the reader, for it lays the basis for a clear understanding of the latter part of the book in which the author discusses social systems, the organization of cognition and of will, the crisis of our present civilization, education and progress, and finally, social immortality. He shows the way in which the desires and thoughts of humanity are woven into social patterns and what direction must be taken in the future if we are to meet the demands for a real democracy.

DIALECTICS OF NATURE.

By Frederick Engels. Translated and Edited by Clemens Dutt with a Preface and Notes by J. B. S. Haldane. International Publishers, New York. $3.00. 8¼ x 5½; xvi + 383; 1940.

Frederick Engels was a much misunderstood genius. His unfortunate partnership with Marx, under which the former did most of the work and the latter got all the credit, has kept Engels pretty much in the backround, so much so that the present work is only now being published in English translation for the first time, more than half a century after it was written.

Engels' object seems to have been to cover the entire field of science. Nothing was too small or insignificant to be worthy of his attention; even psychic phenomena, the authenticity of which he rejected *in toto*, has been given one chapter. As we look back over the progress of science during the past sixty years this treatise seems strangely prophetic. Engels believed that the solar system condensed from a spiral nebula, the planets all having consequently the same temporal age, a belief commonly held today, but which has nothing in common with the nebular hypothesis of Laplace and Kant. The latter believed that the planets were formed in succession by the condensation of rings thrown off from the solar equator by tangential force. Engels propounded his theory at a time when the entire field of astrophysical thought was dominated by Laplace.

In the same way his views on biological theory were far in advance of his time. In the preface of this edition, contributed by J. B. S. Haldane, that writer regrets that he was not earlier acquainted with Engels' comments on Darwin, as they would have spared him much muddled thinking.

Unfortunately Engels, like Franklin, was too early diverted from the pursuit of natural to that of political science, and modern civilization is the poorer from this. Had Engels completed his task of self education in the natural sciences his political conclusions would have had a more solid foundation and would have been correspondingly more

significant and influential. As it is he was a most profound thinker, but his manuscript was left in an extremely sketchy form, is quite ungrammatical, and composed largely of disconnected notes, so that a great deal of editing has been necessary. As a historic document it is full of interest, but as a contribution to modern scientific thought it has not been very successful.

There is a bibliography and an index, but neither seems to be proportionate to a work of this size.

LIFE ON OTHER WORLDS.

By H. Spencer Jones. The Macmillan Co., New York. $3.00. 8 x 5½; x + 299 + 17 plates; 1940.

Does life exist elsewhere than on earth? Today man is better prepared than ever before to answer this question of the ages. Jones has impressively revealed how astronomy, biology, physics, and chemistry have all contributed to the rapid advance in knowledge of our own and other stellar systems. Living things are aggregates of large chain molecules in a colloidal state. Prerequisite for life are the materials and conditions from which such molecules can be formed. A further necessary, but by no means sufficient, condition is the existence of elements and compounds in a physical and chemical state which the organism requires for its metabolic processes. In addition there must be no lethal temperatures, pressures, or chemical compounds. The conditions which exist on a certain world can be estimated from its size and distance from the parent sun, and can generally be confirmed by use of a spectroscope and delicate thermocouple.

One by one Jones eliminates the other planets of our solar system, with the possible exception of Mars where there is strong evidence of vegetation. Indeed, there may at one time have been animal life on Mars. If life does exist elsewhere it need be similar to ours only in its fundamental properties, but modified in form in accordance with the conditions of the other world. The limited bounds within which life is possible would lead us to believe that ours is the only such existence. On the other hand it is difficult to believe that there may not somewhere among the thousands of millions of stars be planetary systems wherein the precise conditions are fulfilled.

SILVER IN INDUSTRY.

Edited by Lawrence Addicks. Reinhold Publishing Corp., New York. $10.00. 9 x 6; vii + 636; 1940.

Among the many subjects covered in this volume biologists will find two of especial interest. The first treats with the oligodynamic effects of silver. The second treats with the fungicidal properties of silver. Carl Nägeli in 1893 showed that pure water brought into contact with clean metallic surfaces was lethal to *spirogyra*. Subsequent analysis showed the concentration of the metal to be of the order of 10^{-8}. The aforementioned effect seems to be due to the presence of metal ions dissolved in the water. Oligodynamic water is not permanent and the lethal property slowly disappears if the source of ions is removed. It has also been found that if the metal concentration is not sufficient for killing, a stimulation occurs. Differences in media, pH, temperature, cell concentration, etc., may account for many of the discrepancies found in the literature. However, it seems that the physiological behavior of the cells must also play an important part. This appears to be particularly true in view of the finding that proteolytic bacteria are generally more resistant than the saccharolytic types.

The volume is well illustrated and indexed. Each chapter has an extensive bibliography appended at the end of the book.

SCIENCE AND THE CLASSICS.

By D'Arcy W. Thompson. Oxford University Press, New York and London. $1.00. 6¾ x 4¼; viii + 264; 1940.

To those who know Thompson only by his contributions to the mathematics of

metabolism this work will come as a delightful surprise, for it reveals a mind of such broad culture that it is equally at home in the fields of astronomy, literature, biography, and the foreign languages. It is a collection of brief essays, each complete in itself, originally delivered as lectures not only at St. Andrew's University, but at others in England and France as well.

The essay on Aristotle is extremely timely, in view of the fact that in another work (also reviewed in this issue) a younger author has seen fit to attempt to deflate this ancient authority in somewhat derogatory tone. The reader who seeks an unprejudiced narration of what Aristotle really accomplished in the way of natural science, and how he surpassed many of his successors, and why he dominated philosophical thought for about twenty centuries will find this all lucidly expounded in this work, which is characterized by its genial humor as well as by its erudition. The discussion of the "Golden Section" is quite striking.

To those who have become stale as the result of too close concentration on specific problems close at hand this book is highly recommended.

THE STORY OF SUPERFINISH.

By Arthur M. Swigert, Jr. Lynn Publishing Co., Detroit. $5.00. 9 x 6; [12] + 672; 1940.

Man has long recognized the truth inherent in the statement that a chain is no stronger than its weakest link. The writer of this book paraphrases the foregoing when he states that a machine is only as efficient as its least satisfactory load-carrying bearing surface. The realization of this has focused the attention of scientists and engineers upon the relationship existing between the boundary layers of metallic surfaces.

The subject is developed from the historical point of view. Correlatives dealing with machine methods and techniques used in testing the surface finishes of materials are given. Among the methods used to test finished surfaces are electronic diffraction, magnaflux, and methods of amplifying the sound of a stylus drawn over the surface.

The many tables and illustrations clarify and amplify the text. No bibliography has been provided and the brief index is quite inadequate.

LOOK AT LIFE! *A Collection of the Nature Photographs.*

By Lynwood M. Chace. Alfred A. Knopf, New York and London. $3.50. 10 x 7; 250; 1940.

Every now and then someone comes along and shows us what we are missing, and this Chace has done with the lens of his camera. This book is a unique collection of nature photographs, but what characterizes these pictures is not mere scientific exactitude, for their maker does not hesitate to pose his subjects sometimes, the better to display them. Much ingenuity has gone into some of his achievements. In certain of the more remarkable photographs in this collection the accuracy of the lens transcends imagination. It becomes the eye of a veritable magician, perceiving that which is hidden from unaided human vision. And Chace has an instinct for significant design. Nothing is too ugly or affrighting, by commonplace standards, for him to look deeper at it, and find its significance and perhaps a revelation of its genuine aesthetic value. Indeed this is a most interesting and absorbing volume.

FRENCH-ENGLISH SCIENCE DICTIONARY *for Students in Agricultural, Biological, and Physical Sciences.*

By Louis De Vries with the Collaboration of Members of the Graduate Faculty. McGraw-Hill Book Co., New York and London. $3.50. 7 x 5; viii + 546; 1940.

Like its companion volume, the *German-English Science Dictionary* by the same author, this compact dictionary of 43,000 entries will prove an invaluable aid to graduate students and workers in the sciences. Terms are included covering

entomology, embryology, cytology, physiology, morphology, genetics, ecology, chemistry, physics, botany and medicine and a general vocabulary. Naturally not all names of animals, insects, plants and chemicals could be included. Many forms of irregular verbs are given with at least one meaning, and for these the infinitive form is marked in parentheses so that the student may consult this verb part for further meanings. This feature will be appreciated by persons with a limited knowledge of French grammer.

VEGETABLE VARIETY: *or How to Enjoy a Meatless Meal.*

By Ann Gurney. Illustrated by Katherine Ogilvy. The Medici Society, London. 1s. 6d. 9 x 5¾; 37; 1940 (paper).

This little book with something over 100 recipes for the cooking of vegetables turns out to be something quite different from what one ordinarily expects an English cook book to be. While one occasionally comes across recipes that call to mind the traditionally poorly cooked English vegetables, a large number of them seem to be well worth trying. Many suggestions are given that will give new life to old dishes. An added interest is given the book by illustrations of old prints and line drawings by Katherine Ogilvy.

OUTDOOR PORTRAITURE. *Problems of Face and Figure in Natural Environment.*

By William Mortensen. Camera Craft Publishing Company, San Francisco. $2.75. 9½ x 6½; 142; 1940.

By far the greatest number of photographs taken by amateurs are outdoor pictures of people. Hence there is need for an authoritative book dealing solely with this subject. Consideration is given first to the mechanism of proper equipment and handling of the camera, then to those bugbears of the amateur—lighting, background, and composition. Both positive and negative suggestions are well depicted by over 100 illustrations.